



MATHEMATICAL
TRACTS

Of the late

Benjamin Robins, Esq;

Fellow of the Royal Society,

AND

Engineer General to the Honourable the East
India Company.





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In TWO VOLUMES.

VOL. II.

Containing his DISCOURSE ON THE METHODS OF
FLUXIONS, and of PRIME AND ULTIMATE RA-
TIOS, with other Miscellaneous Pieces.

Published by JAMES WILSON, M.D.

*Patere honoris scirent ut cunctis viam,
Nec generi tribui, sed virtuti, gloriam,* Phæd.

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ANTHROPOLOGICAL

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THE

VOLUME

II

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A
DISCOURSE

Concerning the
NATURE and CERTAINTY

OF
Sir ISAAC NEWTON'S
METHODS

OF
FLUXIONS,
AND OF

PRIME and ULTIMATE
RATIOS.

First published in 1735.



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INTRO.

INTRODUCTION.

1. **F**ROM many propositions dispersed through the writings of the ancient geometers, and more especially from one whole treatise *, it appears, that the process, by which they investigated the solutions of their problems, was for the most part the reverse of the method, whereby they demonstrated those solutions. But what they have delivered upon the tangents of curve lines, and the mensuration of curvilinear spaces, does not fall under this observation; for the analysis, they made use of in these cases, is no where to be met with in their works. In later times, indeed, a method for investigating such kind of problems has been introduced, by considering all curves, as composed of an infinite number of indivisible straight lines, and curvilinear spaces, as composed in the like manner of parallelograms. But this being an obscure and indistinct conception, it was obnoxious to error.

2. SIR Isaac Newton therefore, to avoid the imperfection, with which this method of indivisibles was justly charged, instituted an analysis for these problems upon other principles. Considering magnitudes not under the notion of being increased by a repeated accession of parts, but as generated by a continued motion or flux; he discovered a method to compare together the velocities, wherewith

* Apollon. de Sectione Rationis, published by Dr. Halley at Oxford in 1706.

homogeneous magnitudes increase, and thereby has taught an analysis free from all obscurity and indistinctness.

3. MOREOVER to facilitate the demonstrations for these kinds of problems, he invented a synthetic form of reasoning from the prime and ultimate ratios of the contemporaneous augments, or decrements of those magnitudes, which is much more concise than the method of demonstrating used in these cases by the ancients, yet is equally distinct and conclusive.

4. OF this analysis, called by Sir Isaac Newton his method of fluxions, and of his doctrine of prime and ultimate ratios, I intend to write in the ensuing discourse. For though Sir Isaac Newton has very distinctly explained both these subjects, the first in his treatise on the Quadrature of curves, and the other in his Mathematical principles of natural philosophy; yet as the author's great brevity has made a more diffusive illustration not altogether unnecessary; I have here endeavoured to consider more at large each of these methods; whereby, I hope, it will appear, they have all the accuracy of the strictest mathematical demonstration.

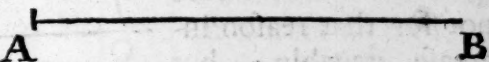


DISCOURSE

OF

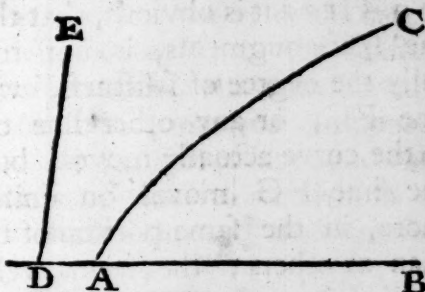
FLUXIONS, &c.

5. **I**N the method of fluxions geometrical magnitudes are not presented to the mind, as compleatly formed at once, but as rising gradually before the imagination by the motion of some of their extremes*.

6. **T**HUS the line AB may be conceived to be traced out gradually by a point moving on from A  B to B, either with an equable motion, or with a velocity in any manner varied. And the velocity, or degree of swiftnes, with which this point moves in any part of the line AB, is called the fluxion of this line at that place.

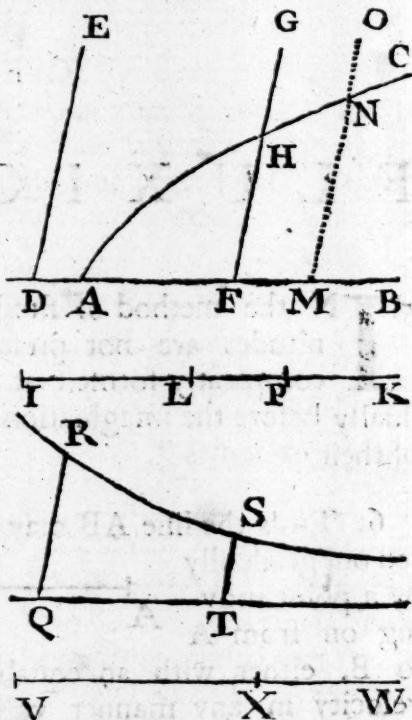
7. **A**GAIN, suppose two lines AB and AC to form a space unbounded towards BC; and upon AB a line DE to be erected.

8. **N**OW, if this line DE be put in motion (suppose so as to keep always parallel to itself,) as soon as it



* Newt. Introd. ad Quad. Curv.

has passed the point A, a space bounded on all sides will begin to appear between these three lines. For instance, when DE is moved into the situation FG, these three lines will include the space AFH. Here it is evident, that this space will increase faster or slower, according to the degree of velocity, wherewith the line DE shall move. It is also evident, that though the line DE should move with an even pace, the space AFH would not for that reason increase equably; but where the line AC was farthest distant from AB, the space AFH would increase fastest. Now the velocity or celerity, wherewith the space AFH at all times increases, is called the fluxion of that space.



9. HERE it is obvious, that the velocity, wherewith the space augments, is not to be understood literally the degree of swiftness, with which either the line FG, or any other line or point appertaining to the curve actually moves; but as this space, while the line FG moves on uniformly, will increase more, in the same portion of time, at some places, than at others; the terms velocity and celerity are applied in a figurative sense to denote the degree, wherewith this augmentation in every part proceeds.

10. BUT

10. But we may divest the consideration of the fluxion of the space from this figurative phrase, by causing a point to pass over any straight line IK, that the length IL measured out, while the line DE is moving from A to F, shall augment in the same proportion with the space AFH. For this line being thus described faster or slower in the same proportion, as the space receives its augmentation; the velocity or degree of swiftness, wherewith the point describing this line actually moves, will mark out the degree of celerity, wherewith the space every where increases. And here the line IL will preserve always the same analogy to the space AFH; in so much, that, when the line DE is advanced into any other situation MNO, if IP be to IL in the proportion of the space AMN to the space AFH, the fluxion of the space at MN will be to the fluxion thereof at FH, as the velocity, wherewith the point describing the line IK moves at P, to the velocity of the same at L. And if any other space QRST be described along with the former by the like motion, and at the same time a line VW, so that the portion VX shall always have to the length IL the same proportion, as the space QRST bears to the space AFH; the fluxion of this latter space at TS will be to the fluxion of the former at FH, as the velocity, wherewith the line VW is described at X, to the velocity, wherewith IK is described at L. It will hereafter appear, that in all the applications of fluxions to geometrical problems, where spaces are concerned, nothing more is necessary, than to determine the velocity, wherewith such lines as these are described*.

* §. 49.

11. IN the same manner may a solid space be conceived to augment with a continual flux, by the motion of some plane, whereby it is bounded; and the velocity of its augmentation (which may be estimated in like manner) will be the fluxion of that solid.

12. FLUXIONS then in general are the velocities, with which magnitudes varying by a continued motion increase or diminish; and the magnitudes themselves are reciprocally called the fluents of those fluxions *.

13. AND as different fluents may be understood to be described together in such manner, as constantly to preserve some one known relation to each other; the doctrine of fluxions teaches, how to assign at all times the proportion between the velocities, wherewith homogeneous magnitudes, varying thus together, augment or diminish.

14. THIS doctrine also teaches on the other hand, how, from the relation known between the fluxions, to discover what relation the fluents themselves bear to each other.

15. IT is by means of this proportion only, that fluxions are applied to geometrical uses; for this doctrine never requires any determinate degree of velocity to be assigned for the fluxion of any one fluent. And that the proportion between the fluxions of magnitudes is assignable from the relation known between the magnitudes themselves, I now proceed to shew.

* *Motuum vel incrementorum velocitates nominando fluxiones, & quantitates genitas nominando fluentes. Newton. Introd. ad Quadr. Curv.*

16. IN the first place, let us suppose two lines AB and CD to be

$A \quad \quad \quad I \quad E \quad G \quad \quad \quad B$
 $\quad \quad \quad | \quad | \quad |$
 $C \quad \quad \quad K \quad F \quad H \quad \quad \quad D$
 $\quad \quad \quad | \quad | \quad |$

described together by two points, one set-

ting out from A, and the other from C, and to move in such manner, that if AE and CF are lengths described in the same time, CF shall be analogous to some power of AE, that is, if AE be denoted by the letter x , then CF shall always be denoted by $\frac{x^n}{a^{n-1}}$; where a represents some given line, and n any number whatever. Here, I say, the proportion between the velocity of the point moving on AB to the velocity of that moving on CD, is at all times assignable.

17. FOR let any other situations, that these moving points shall have at the same instant of time, be taken, either farther advanced from E and F, as at G and H, or short of the same, as at I and K; then if EG be denoted by e , CH, the length passed over by the point moving on the line CD, while the point in the line AB has passed from A to G, will be expressed by $\frac{x+e}{a^{n-1}}^n$; and if EI be denoted by e , CK, the length passed over by the point moving on the line CD, while the point moving in AB has got only to I, will be denoted by $\frac{x-e}{a^{n-1}}^n$: or reducing each of these terms into a

series, CH will be denoted by $\frac{x^n}{a^{n-1}} + \frac{nx^{n-1}e}{a^{n-1}}$

$+ \frac{n \times n-1, x^{n-2}ee}{2a^{n-1}} + \frac{n \times n-1 \times n-2, x^{n-3}e^3}{6a^{n-1}} +$

&c.

&c. and CK by $\frac{x^n}{a^{n-1}} - \frac{nx^{n-1}e}{a^{n-1}} + \frac{n \times n-1 \times x^{n-2}ee}{2a^{n-1}}$
 $-\frac{n \times n-1 \times n-2 \times x^{n-3}e^3}{6a^{n-1}} + \&c.$ Hence all the
 terms of the former series, except the first term, viz.
 $\frac{nx^{n-1}e}{a^{n-1}} - \frac{n \times n-1 \times x^{n-2}ee}{2a^{n-1}} + \&c.$ will denote FH;
 and all the latter series, except the first term, viz.
 $\frac{nx^{n-2}ee}{2a^{n-1}} - \frac{n \times n-1 \times x^{n-3}e^3}{6a^{n-1}} + \&c.$ will denote KF.

18. WHEN the number n is greater than unite, while the line AB is described with a uniform motion, the point, wherewith CD is described, moves with a velocity continually accelerated; for if IE be equal to EG, FH will be greater than KF.

19. Now here, I say, that neither the proportion of FH to EG, nor the proportion of KF to IE is the proportion of the velocity, which the point moving on CD has at F, to the uniform velocity, wherewith the point moves on the line AB. For, while that point is advanced from E to G, the point moving on CD has passed from F to H, and has moved through that space with a velocity continually accelerated; therefore, if it had moved during the same interval of time with the velocity, it has at F, uniformly continued, it would not have passed over so long a line; consequently FH bears a greater proportion to EG, than what the velocity, which the point moving on CD has at F, bears to the velocity of the point moving uniformly on AB.

20. IN

20. IN like manner KF bears to IE a less proportion than that, which the velocity of the point in CD has at F, to the velocity of that in AB. For as the point in CD, in moving from K to F, proceeds with a velocity continually accelerated; with the velocity, it has acquired at F, if uniformly continued, it would describe in the same space of time a line longer than KF.

21. IN the last place I say, that no line whatever, that shall be greater or less than the line represented by the second term of the foregoing series (*viz.*

the term $\frac{nx^{n-1}e}{a^{n-1}}$) will bear to the line denoted

by e the same proportion, as the velocity, wherewith the point moves at F, bears to the velocity of the point moving in the line AB; but that the ve-

locity at F is to that at E as $\frac{nx^{n-1}e}{a^{n-1}}$ to e , or as nx^{n-1} to a^{n-1} .

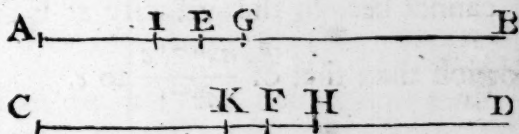
22. IF possible let the velocity at F bear to the velocity at E a greater ratio than this, suppose the ratio of p to q .

23. IN the series, whereby CH is denoted, the line e can be taken so small, that any term proposed in the series

shall exceed all the following terms together; so that the double of

that term shall be greater than the whole collection of that term, and all that follow. Again, by diminishing

e ,



e , the ratio of the second term in this series, to twice

the third, that is, of $\frac{nx^{n-1}e}{a^{n-1}}$ to $\frac{n \times n-1 x^{n-2}ee}{a^{n-1}}$, or

the ratio of x to $n-1 \times e$, may be rendered greater than any, that shall be proposed; consequently the line e may be taken so small, that twice the third

term, that is $\frac{n \times n-1 x^{n-2}ee}{a^{n-1}}$, shall be greater than

all the terms following the second, and also, that the

ratio of $\frac{nx^{n-1}e}{a^{n-1}} + \frac{n \times n-1 x^{n-2}ee}{a^{n-1}}$ to e shall less ex-

ceed the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to e , than any other ratio

whatever, that may be proposed. Therefore let the

ratio of $\frac{nx^{n-1}e}{a^{n-1}} + \frac{n \times n-1 x^{n-2}ee}{a^{n-1}}$ to e be less than the

ratio of p to q ; then, if $\frac{n \times n-1 x^{n-2}ee}{a^{n-1}}$ be also great-

er than the third and all the following terms of the

series, the ratio of the series $\frac{nx^{n-1}e}{a^{n-1}} + \frac{n \times n-1 x^{n-2}ee}{2a^{n-1}}$

+ &c. to e , that is, the ratio of FH to EG shall be less than the ratio of p to q , or of the velocity at F to the velocity at E , which is absurd; for it has above been shewn, that the first of these ratios is greater than the last. Therefore the velocity at F cannot bear to the velocity at E any greater pro-

portion than that of $\frac{nx^{n-1}e}{a^{n-1}}$ to e .

24. On the other hand, if possible, let the velocity at F bear to the velocity at E a less ratio than that

that of $\frac{nx^{n-1}e}{a^{n-1}}$ to e : let this lesser ratio be that of r to s .

25. IN the series whereby CK is denoted, e may be taken so small, that any one term proposed shall exceed the whole sum of all the following terms, when added together. Therefore let e be taken so

small, that the third term $\frac{n \times n-1 x^{n-2} ee}{2a^{n-1}}$ ex-

ceed all the following terms $\frac{n \times n-1 \times n-2 x^{n-3} e^3}{6a^{n-1}}$,

$\frac{n \times n-1 \times n-2 \times n-3 x^{n-4} e^4}{24a^{n-1}}$, &c. added together.

But e may also be rendered so small, that the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to $\frac{n \times n-1 x^{n-2} ee}{a^{n-1}}$, the double of the third term, shall be greater than any ratio whatever that shall be proposed; and the ratio of $\frac{nx^{n-1}e}{a^{n-1}}$

$\frac{n \times n-1 x^{n-2} ee}{a^{n-1}}$ to e shall come less short of the

ratio of $\frac{nx^{n-1}e}{a^{n-1}}$ to e , than any other ratio, that can be named. Therefore let this ratio exceed the ratio

of r to s ; then the term $\frac{n \times n-1 x^{n-2} ee}{2a^{n-1}}$ exceed-

ing the whole sum of all the following terms in the series denoting CK, the whole series

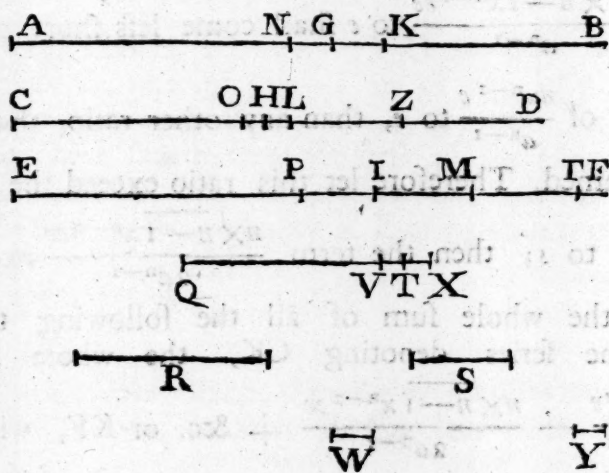
$\frac{nx^{n-1}e}{a^{n-1}} - \frac{n \times n-1 x^{n-2} ee}{2a^{n-1}} + \&c.$ or KF, will in

every case bear to e , or EI a greater ratio than that of r to s , or of the velocity at F to the velocity at E , which is absurd. For it has above been shewn, that the first of these ratios is less than the last.

26. If n be less than unite, the point in the line CD moves with a velocity continually decreasing; and if n be a negative number, this point moves backwards. But in all these cases the demonstration proceeds in like manner.

27. Thus have we here made appear, that from the relation between the lines AE and CF , the proportion between the velocities, wherewith they are described, is discoverable; for we have shewn, that the proportion of nx^{n-1} to a^{n-1} is the true proportion of the velocity, wherewith CF , or $\frac{x^n}{a^{n-1}}$ augments, to the velocity, wherewith AE or x is at the same time augmented.

28. AGAIN, in the three lines AB , CD , EF ,



where

where the points A, C, E are given, let us suppose G, H and I to be three contemporary positions of the points, whereby the three lines AB, CD, EF are respectively described; and let the motion of the point describing the line EF be so regulated with regard to the motion of the other two points, that the rectangle under EI and some given line may be always equal to the rectangle under AG and CH. Here from the velocities, or degrees of swiftness, wherewith the points describing AB and CD move, the degree of swiftness, wherewith the point describing EF moves, may be determined.

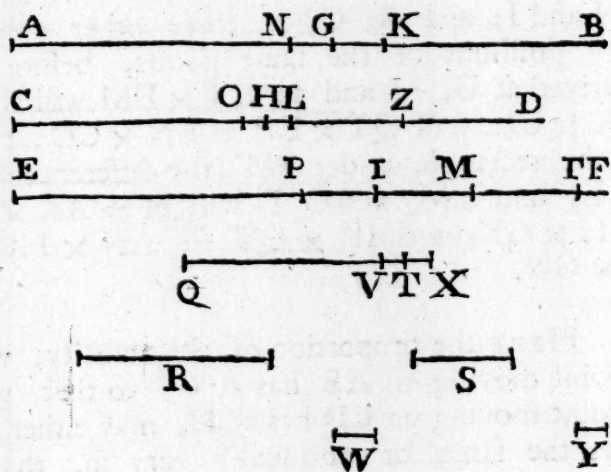
29. THE points moving on the lines AB, CD may either move both the same way, or one forwards and the other backwards.

30. In the first place suppose them to move the same way, advancing forward from A and C; and since some given line forms with EI a rectangle equal to that under AG and CH, suppose $QT \times EI = AG \times CH$: then, if K, L, M are contemporary positions of the points moving on the lines AB, CD, EF, when advanced forward beyond G, H and I; and N, O, P, three other contemporary positions of the same points, before they are arrived at G, H and I; $QT \times EM$ will also be $= AK \times CL$, and $QT \times EP = AN \times CO$; therefore the rectangle under IM (the difference of the lines EI and EM) and QT will be $= AK \times HL + CH \times GK$, and $IP \times QT = AN \times HO + CH \times GN$.

31. HERE the proportion of the velocity, which the point moving on AB has at G, to that, which the point moving on CD has at H, may either keep always the same, or continually vary so, that one

of these velocities (suppose that of the point moving on the line CD) shall have to the other a proportion gradually augmenting; that is, if NG and GK are equal, HL shall either be equal to OH or greater. Here, since $IM \times QT = AK \times HL + CH \times GK$, and $IP \times QT = AN \times HO + CH \times GN$, where $CH \times GK = CH \times GN$ and $AK \times HL$ in both cases greater than $AN \times HO$, IM will be greater than IP; in so much that in both these cases the velocity of the point, where-with the line EF is described, will have to the velocity of the point moving on AB a proportion, gradually augmenting. Here therefore the line IM will bear to GK a greater proportion, than the velocity of the point moving on the line EF, when at I, bears to the velocity of the point moving on the line AB, when at G; and the line PI will have a less proportion to NG, than the velocity, which the point moving on the line EF, has at I, to the velocity, which the point moving on the line AB has at G.

32. Now let R be to S as the velocity, which the



point

point moving on AB has at G, to the velocity, which the point moving on CD has at H; then I say, that the velocity, which the point moving on EF has at I, will be to the velocity, which the point moving on AB has at G, as $AG \times S + CH \times R$ to $QT \times R$.

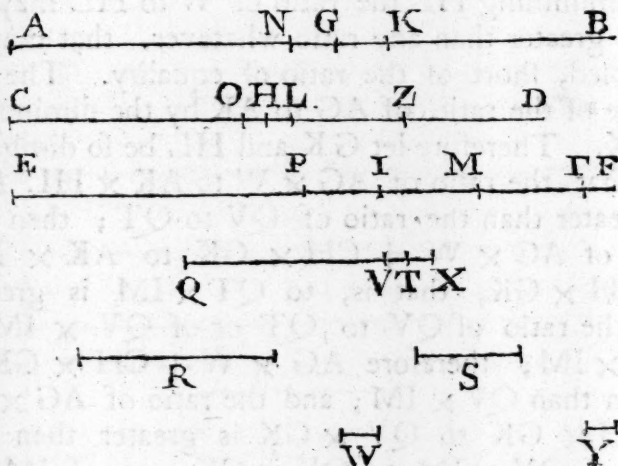
33. If possible let the velocity, which the point moving on EF has at I, be to the velocity, which the point moving on AB has at G, as $AG \times S + CH \times R$ to the rectangle under R and some line QV less than QT.

34. TAKE W to GK in the ratio of S to R; then will $AG \times S + CH \times R$ be to $R \times QV$ as $AG \times W + CH \times GK$ to $QV \times GK$. Here, because the ratio of the velocity of the point moving on the line CD to the velocity of the point moving on AB either remains constantly the same, or gradually augments, W is either equal to HL or less; but when it is less, by diminishing HL the ratio of W to HL may become greater than any ratio whatever, that may be proposed, short of the ratio of equality. The like is true of the ratio of AG to AK by the diminution of GK. Therefore let GK and HL be so diminished, that the ratio of $AG \times W$ to $AK \times HL$ shall be greater than the ratio of QV to QT; then the ratio of $AG \times W + CH \times GK$ to $AK \times HL + CH \times GK$, that is, to $QT \times IM$ is greater than the ratio of QV to QT or of $QV \times IM$ to $QT \times IM$; therefore $AG \times W + CH \times GK$ is greater than $QV \times IM$; and the ratio of $AG \times W + CH \times GK$ to $QV \times GK$ is greater than the ratio of $QV \times IM$ to $QV \times GK$, or of IM to GK; but the ratio of IM to GK is greater than that of the velocity, which the point moving on EF has at I, to the velocity, which the point moving on AB has at G; therefore the ratio of $AG \times W$

+ $CH \times GK$ to $QV \times GK$, or that of $AG \times S$ + $CH \times R$ to $QV \times R$, still more exceeds the ratio of the velocity at I to the velocity at G; and consequently the ratio of the velocity at I to the velocity at G is not greater than that of $AG \times S$ + $CH \times R$ to $QT \times R$.

35. AGAIN, if possible let the velocity, which the point moving on EF has at I, be to the velocity, which the point moving on AB has at G, as $AG \times S$ + $CH \times R$ to the rectangle under R and some line QX greater than QT.

36. HERE let Y be to NG as S to R; then will $AG \times S$ + $CH \times R$ be to $R \times QX$ as $AG \times Y$ + $CH \times NG$ to $QX \times NG$. But Y will be either greater than HO, or equal to it, and when greater, by diminishing HO, the ratio of Y to HO may become less than any ratio whatever, that shall be proposed, greater than the ratio of equality. The like is



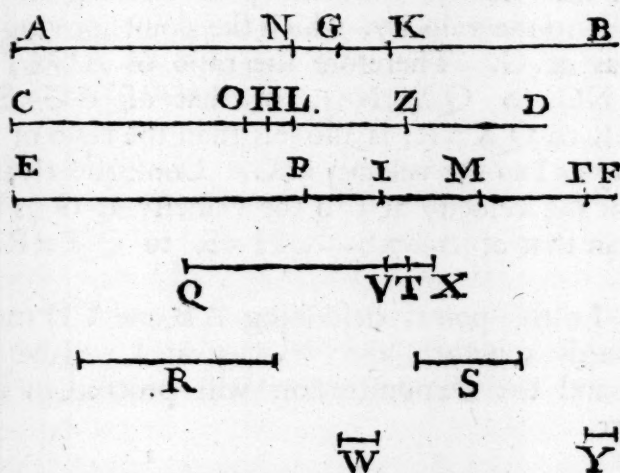
true of the ratio of AG to AN by the diminution of NG. Therefore let NG and HO be so diminished, that the ratio of $AG \times Y$ to $AN \times HO$ shall be

be less than the ratio of QX to QT ; then the ratio of $AG \times Y + CH \times NG$ to $AN \times HO + CH \times NG$, that is, to $QT \times IP$, is less than the ratio of QX to QT , or of $QX \times IP$ to $QT \times IP$. Consequently $AG \times Y + CH \times NG$ is less than $QX \times IP$, and the ratio of $AG \times Y + CH \times NG$ to $QX \times NG$ is less than the ratio of $QX \times IP$ to $QX \times NG$, or of IP to NG . But the ratio of IP to NG is less than that of the velocity, which the point moving on EF has at I , to the velocity, which the point moving on AB has at G . Therefore the ratio of $AG \times Y + CH \times NG$ to $QX \times NG$, or that of $AG \times S + CH \times R$ to $QX \times R$, is also less than the ratio of the velocity at I to the velocity at G . Consequently, the ratio of the velocity at I to the velocity at G is not less than that of $AG \times S + CH \times R$ to $QT \times R$.

37. IF the points describing AB and CD move backwards together, the velocity at I will be the same, and the demonstration will proceed in like manner.

38. BUT if one of the points, as that moving on CD , recedes, while the other on AB advances forward, take in CD any fix'd point at pleasure Z ; then the point on CD in respect of Z moves also forward. Again, take in the line EF , $E\Gamma$ to AG as CZ to QT ; then $AG \times CZ$ is $= QT \times E\Gamma$; and $AG \times CH$ being $= QT \times EI$, $AG \times HZ$ will be $= QT \times FI$; and by the preceding case $AG \times S + ZH \times R$ will be to $QT \times R$ as the velocity, wherewith the point moving on EF separates from Γ , when at I , to the velocity, which the point moving on AB has at G . But as AG is continually increasing, and EF keeps always in the same proportion to AG ; the point Γ will itself be in motion, and the velocity of the point Γ will be to the velocity at G , as the line EF to AG , that is, as CZ to QT ,

QT, or as $CZ \times R$ to $QT \times R$; therefore the velocity, wherewith the point moving on EF, when at I, separates from F, being to the velocity of the point moving on AB, when at G, as $AG \times S + ZH \times R$ to $QT \times R$; the absolute velocity, which the point moving on EF has at I, will be to the absolute velocity, which the point moving on AB has at G, as $AG \times S \cup CH \times R$ to $QT \times R$; moving backwards,



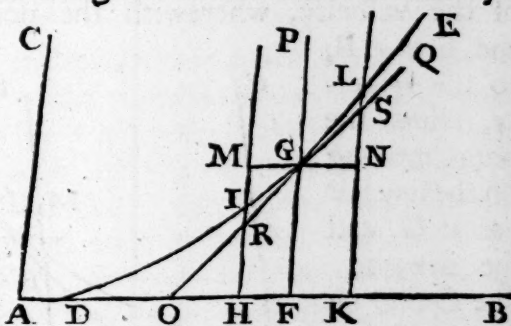
when it separates from F swifter than the point F itself moves, that is, when $AG \times S + ZH \times R$ is greater than $CZ \times R$, or $AG \times S$ greater than $CH \times R$; and when the point moving on EF, at I separates from F with a slower motion, than that wherewith F moves, that is, when $CZ \times R$ is greater than $AG \times S + ZH \times R$, or $AG \times S$ less than $CH \times R$, the point moving on EF, at I advances forward.

39. IN these demonstrations the fluxions of lines only have been considered; but by these the fluxions of all other quantities are determined. For we have already observed, that the fluxions of spaces, whether superficial or solid, are analogous to the velocities, wherewith lines are described, that augment in the same proportion with such spaces. 40. THUS

40. THUS we have attempted to prove the truth of the rules, Sir Isaac Newton has laid down, for finding the fluxions of quantities, by demonstrating the two cases, on which all the rest depend, after a method, which from all antiquity has been allowed as genuine, and universally acknowledged to be free from the least shadow of uncertainty.

41. WE shall hereafter * endeavour to make manifest, that Sir Isaac Newton's own demonstrations are equally just with these here exhibited. But first we shall prove, that in all the applications of this doctrine to the solution of geometrical problems, no other conception concerning fluxions is necessary, than what we have here given. And for this end it will be sufficient to shew, how fluxions are to be applied to the drawing of tangents to curve lines, and to the mensuration of curvilinear spaces.

42. IF upon the line AB be erected in any angle another streight line AC, and it be put in motion upon the line AB towards B keeping always parallel to itself, and proceeding on with a uniform velocity: if a point also moves on the line AC with a velocity in any manner regulated; this point will describe within the angle under CAB some third line DE, which will be a curve, unless the point moves in the line AC likewise with a uniform motion.



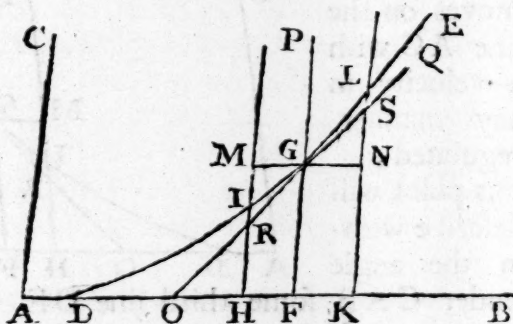
* §. 144.

43. HERE,

43. HERE, I say, the line AC being advanced to any situation FG, by what has already been written on the nature of fluxions, without any adventitious consideration whatever, a tangent may be assigned to the curve at the point G.

44. WHEN the point moves on the line AC with an accelerating velocity, the curve DE will be convex to the abscisse DB. Now if two other situations HI and KL of the line AC be taken, one on each side FG, and MGN be drawn parallel to AB; while the line AC is moving from the situation HI to FG, the point in it will have moved through the length IM, and while the same line AC moves from FG to KL, the point in it will have passed over the length NL. And since the point moves with an accelerated velocity, IM will be less, and NL greater than the space, which would have been described in the same time by the velocity, the point has at G.

45. LET FO be taken to FG in the proportion of the velocity; wherewith the point F moves on the line AB, to the velocity, which the point moving on the line FP has at G, and the straight line OGQ be drawn, cutting HI in R, and KL in S; then FH will be to MR, and FK to NS in the same proportion. Therefore, from what has been said above, MR will be greater than MI, and NS less than NL; so that the line OQ, which
unites



unites with the curve at the point G, lies on both sides the point G, on the same side of the curve; that is, it does not cross, or cut the curve (as geometers speak) but touches it only at the point G.

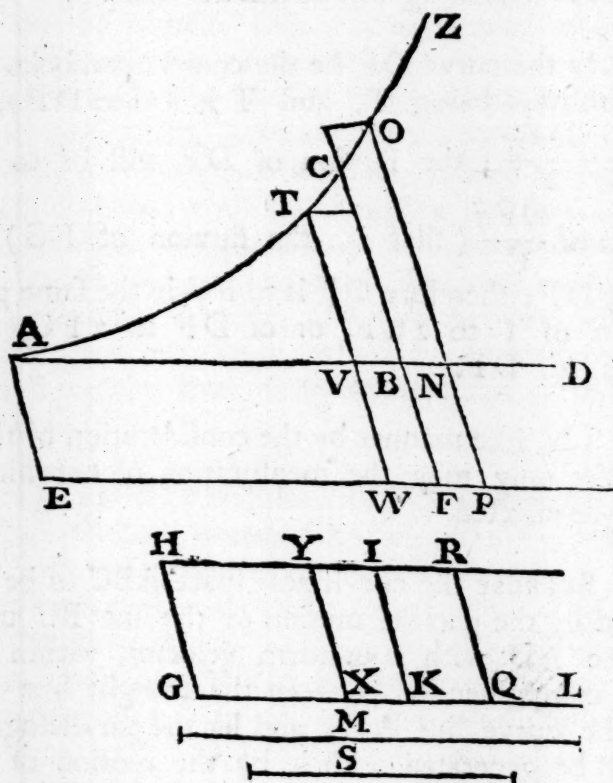
46. WHEN the point moves on the line AC with a velocity gradually decreasing, the curve will be concave towards the abscissa; but in this case the method of reasoning will be still the same.

47. IF the curve DE be the conical parabola, the latus rectum being T, and $T \times FG = DF^2$, or $FG = \frac{DF^2}{T}$; the fluxion of DF will be to the fluxion of $\frac{DF^2}{T}$ (that is, the fluxion of FG) as T to 2 DF; therefore OF is to FG in the same proportion of T to 2 DF, or of DF to 2 FG, and OF is half DF.

48. IN like manner by the consideration of these velocities only may the mensuration of curvilinear spaces be effected.

49. SUPPOSE the curvilinear space ABC to be generated by the parallel motion of the line BC upon the line AD with a uniform velocity, within the space comprehended between the straight line AD and the curve line AZ; and let the parallelogram AEFB be generated with it by the motion of BF accompanying BC. Suppose another parallelogram GHIK to be generated at the same time by the motion of the line GH equal to AE or BF, insinuating on the line GL in an angle equal to that under CBD; and let the motion of GH be so regulated, that the parallelogram GHIK be always equal to the

the curvilinear space ABC . Then it is evident, by what has been said above in our explanation of the nature of fluxions, that the velocity, wherewith the parallelogram $EABF$ increases, is to the velocity, wherewith the parallelogram $GHIK$, or wherewith the curvilinear space ABC increases; as the velocity, wherewith the point B moves, to the velocity, wherewith the point K moves.



50. Now I say, the velocity of the point B is to the velocity of the point K as BF to BC .

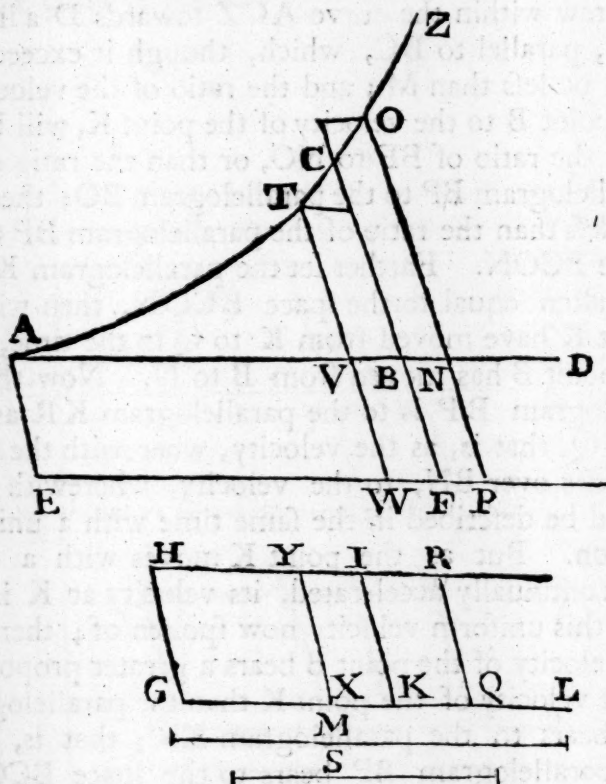
51. SUPPOSE the curve line ACZ to recede farther and farther from AD ; then it is evident, that while the parallelogram $EABF$ augments uniformly, the curvilinear space ABC will increase faster and faster;

faster; therefore in this case the point K moves with a velocity continually accelerated.

52. HERE, if possible, suppose the velocity of the point B to bear a less proportion to the velocity of the point K, than the ratio of BF to BC; that is, let the velocity of B be to the velocity of K, as BF to some line M greater than BC. Then it is possible to draw within the curve ACZ towards D a line, as ON, parallel to BC, which, though it exceed BC, shall be less than M; and the ratio of the velocity of the point B to the velocity of the point K, will be less than the ratio of BF to NO, or than the ratio of the parallelogram BP to the parallelogram BO; therefore still less than the ratio of the parallelogram BP to the space BCON. Farther let the parallelogram KIRQ be taken equal to the space BCON, then will the point K have moved from K to Q in the time, that the point B has moved from B to N. Now the parallelogram BP is to the parallelogram KR as BN to KQ, that is, as the velocity, wherewith the point B passes over BN, to the velocity, wherewith KQ would be described in the same time with a uniform motion. But as the point K moves with a velocity continually accelerated, its velocity at K is less than this uniform velocity now spoken of; therefore the velocity of the point B bears a greater proportion to the velocity of the point K than the parallelogram BP bears to the parallelogram KR; that is, than the parallelogram BP bears to the space BCON; but the first of these ratios was before found less than the last; which involves an absurdity. Therefore the velocity of B bears not to the velocity of K a less proportion than that of BF to BC.

53. AGAIN, if possible, let the velocity of B bear to the velocity of K a greater proportion than that of BF to BC, that is, the proportion of BF to some

some line S less than BC ; and let the line TV be drawn parallel to CB , and greater than S , and the parallelogram TB be compleated. Here the ratio of the velocity of the point B to the velocity of the point K will be greater than the ratio of BF to TV , or than the ratio of the parallelogram BW to the parallelogram BT , therefore still greater than the ratio of the parallelogram BW to the curvilinear



space VTB . Now if the parallelogram $XYIK$ be taken equal to the space VTB , that the point describing the line GL may have moved from X to K , while VT has moved to BC ; since the parallelogram BW is to the parallelogram XI as VB to XK , that is, as the velocity, wherewith the point B has passed over VB , to the velocity, wherewith XK would

would be described in the same time with a uniform motion, the velocity of the point B bears a less proportion to the velocity of the point K, than the parallelogram BW bears to the parallelogram XI, because XK is described with an accelerating velocity: that is, the velocity of the point B bears a less proportion to the velocity of the point K, than the parallelogram BW bears to the space VTCB. But the first of those ratios was before found greater than the last. Therefore the velocity of B does not bear to the velocity of K a greater proportion than that of BF to BC.

54. IF the curve line ACZ were of any other form, the demonstration would still proceed in the same manner.

55. HENCE it appears, that nothing more is necessary towards the mensuration of the curvilinear space ABC, than to find a line GK so related to AB, that, while they are described together, the velocity of the point, wherewith AB is described, shall bear the same proportion at any place B to the velocity, wherewith the point describing the other line GK moves at the correspondent place K, as some given line AE bears to the ordinate BC of the curve ACZ.

56. THE method of finding such lines is the subject of Sir Isaac Newton's Treatise upon the Quadrature of Curves.

57. FOR example, if ACZ be a conical parabola as before, and $\Gamma \times BC = ABq$; taking $GK = \frac{ABc}{3\Gamma \times GH}$, the parallelogram $HK = \frac{ABc}{3\Gamma} = \frac{1}{3} AB \times BC$, is equal to the space ABC; for GK being equal to $\frac{ABc}{3\Gamma \times GH}$, the fluxion of GK or the velocity

velocity, wherewith it is described at K, will be to the fluxion of AB, or the velocity, wherewith B moves, as $\frac{ABq}{I}$ or BC to GH or AE.

58. HAVING thus, as we conceive, sufficiently explained, what relates to the proportions between the velocities wherewith magnitudes are generated; nothing now remains, before we proceed to the second part of our present design, but to consider the variations, to which these velocities are subject.

59. WHEN fluents are not augmented by a uniform velocity, it is convenient in many problems to consider how these velocities vary. This variation Sir Isaac Newton calls the fluxion of the fluxion, and also the second fluxion of the fluent; distinguishing the fluxions, we have hitherto treated of, by the name of the first fluxions. The second fluxions may also vary in different magnitudes of the fluent, and the variation of these is called the third fluxion of the fluent. Fourth fluxions are the changes to which the third are subject, and so on *.

60. IN the two fluents AE and CF, whose fluxions we compared at §. 15, &c. where AE being denominated by x ,

CF was equal $A, \overset{I}{\mid} \overset{E}{\mid} \overset{G}{\mid} \text{---} B$
to $\frac{x^n}{n^{n-1}}$, and $C, \text{---} \overset{K}{\mid} \overset{F}{\mid} \overset{H}{\mid} D$
the fluxion of

AE bore to the fluxion of CF the proportion of n^{n-1} to nx^{n-1} . Here it is evident, that the antecedent n^{n-1} of this proportion being a fix'd quantity, and the consequent nx^{n-1} a variable one; the fluxion

* Fluxionum (scilicet primarum) fluxiones seu mutationes magis aut minus celeres fluxiones secundas nominare licet, &c. Newt. Quadr. Curv. in Princip.

of AE does not bear to the fluxion of CF always the same proportion. If n be the number 2, the fluxion of AE is to the fluxion of CF as a to the variable quantity $2x$; and if n be the number 3, the fluxion of AE to that of CF will be as a^2 to $3x^2$. Therefore if AE be described with a uniform velocity, when n is any number greater than unite, CF is so described with a velocity continually accelerating, that when n is $= 2$, this velocity augments in the same proportion as CF itself increases; and when n is $= 3$, it augments in the duplicate of that proportion, &c.

61. HERE therefore we see, that while one quantity flows uniformly, the other is described with a varying motion; and the variation in this motion is called the second fluxion of this quantity.

62. IT is evident farther, that in this instance, when n is $= 2$, the variation of the velocity is uniform; for the velocity keeping always in the same proportion to x , while x increases uniformly, the velocity must also increase after the same manner. But when n is $= 3$; since the velocity is every where as x^2 , and x^2 does not increase uniformly; neither will the velocity augment uniformly. So that it appears by this example, that the variation in the velocity, wherewith magnitudes increase, may also vary, and this variation is called the third fluxion of the magnitude.

63. IN the same manner may the fluxions of the following orders be conceived; each order being the variation found in the preceding one. And the consideration of velocities thus perpetually varying, and their variation itself changing, is a useful speculation; for most, if not all, the bodies, we have any

acquaintance with, do actually move with velocities thus modified.

64. A STONE, for instance, in its direct fall towards the earth has its velocity perpetually augmented; and in Galileo's Theory of falling Bodies, when the whole descent is performed near the surface of the earth, it is supposed to receive equal augmentations of velocity in equal times. In this case therefore the velocity augments uniformly, and the second fluxion of the line described by the falling body will in all parts of that line be the same; so that third fluxions cannot take place in this instance; since the variation of the velocity suffers no change, but is every where uniform.

65. BUT if the stone be supposed to have its gravity at the beginning of its fall less than at the surface of the earth, the variation of its velocity at first will then be less than the variation toward the end of its motion; or in other words, the second fluxions in the beginning and end of its fall would be unequal; consequently, third fluxions would here take place, since the variation would be swifter, as the body in its fall approached the earth.

66. THE stone in this last instance then not only moves with a velocity perpetually varying, as in the preceding example, but this variation continually changes. In the true theory of falling bodies, neither this last variation nor any subsequent one can ever be uniform; so that fluxions of every order do here actually exist.

67. THE same is true of the motion of the planets in their elliptic orbs; of the motion of light at the confines of different mediums, and of the motion of all pendulous bodies.

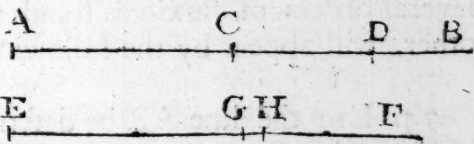
68. IN

68. IN short, a uniform unchangeable velocity is not to be met with in any of those bodies, that fall under our cognizance; for in order to continue such a motion as this, it is necessary, that they should not be disturbed by any force whatever, either of impulse or resistance; but we know of no spaces, in which at least one of these causes of variation does not operate.

69. HAVING thus explained the general conception of second, third, and following fluxions; and having shewn, that they are applicable to the circumstances, which do really occur in all motion, we are acquainted with; we will now endeavour to declare the manner of assigning them.

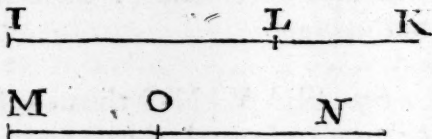
70. AND in the first place second fluxions may be compared together as follows. Suppose any line to be so described by motion, that it always preserve the same analogy to the first fluxion of any magnitude; then the velocity, wherewith this line is described, that is, the fluxion of this line, will be analogous to the second fluxion of the aforefaid magnitude. For it is evident, that this line will perpetually alter in magnitude in the same proportion, as the fluxion, to which it is analogous, varies.

71. SUPPOSE AB to be a fluent described with a varying motion; the second fluxion at any one point C may be compared with the second fluxion at any other point D, by causing the line EF to be described by the motion of a point, so as to keep always the same analogy to the first fluxion of the fluent AB. Suppose EG be to EH, as the



first fluxion at C to the first fluxion at D; then the second fluxion at C will be to the second fluxion at D, as the first fluxion of the line EF at G, to the first fluxion of the same at H.

72. IN like manner, if another fluent IK be generated along with the former fluent AB, and also described with a variable motion; the second fluxion of this latter fluent IK at any place L may be compared



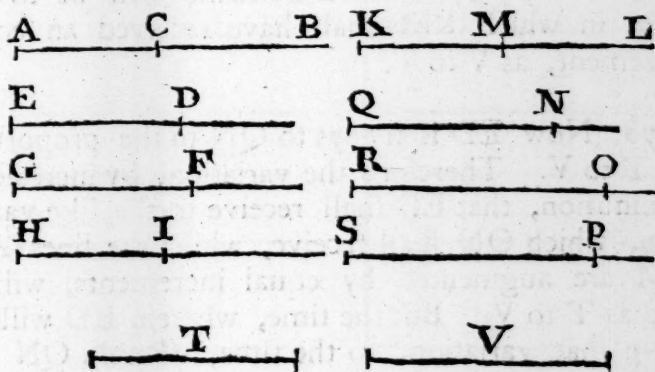
with the second fluxion at any part of the former fluent AB, by describing the line MN with such a motion, as always to preserve the same analogy to the first fluxion of the fluent IK, as the line EF bore to the first fluxion of AB. Suppose MO to be to EG, as the first fluxion of IK at L to the first fluxion of AB at C; then the second fluxion at L will be to the second fluxion at C, as the velocity, wherewith the line MN is described at O, to the velocity, wherewith the line EF is described at G.

73. IN the same manner if a line be described analogous to the second fluxion of any magnitude, the fluxion of this line will express the third fluxion of that magnitude, and so of all the other orders of fluxions.

74. IN the next place the relation, in which the several orders of fluxions stand with regard to each other, will appear by the following proposition.

75. LET the line AB be described by the motion of the point C moving with a varying velocity, and let a series of lines be adapted to this line AB in such manner, that the point D moving upon the first line of this series at the same time with the point C, may
ever

ever terminate a line ED analogous to the velocity of the point C; the point F at the same time terminating upon the second line of this series a line GF analogous to the velocity of the point D; and HI upon the third line being by the motion of the point I made ever analogous to the velocity of the point F; &c.



76. If now another line KL be described by the motion of the point M, and if a series of lines be adapted to this line KL in the like analogy by the motion of the points N, O, P, so that QN be to ED as the velocity of the point M to the velocity of the point C, RO to GF as the velocity of the point N to that of the point D, and SP to HI as the velocity of the point O to that of F; I say, that if the velocity of the point C has to the velocity of the point M always the same proportion at equal distances from A and K, that then the velocity of D to that of N will be in the duplicate of that proportion; the velocity of F to that of O in the triplicate of that proportion; the velocity of I to that of P in the quadruplicate of that proportion, and so on in the same order, as far as these series of lines are extended.

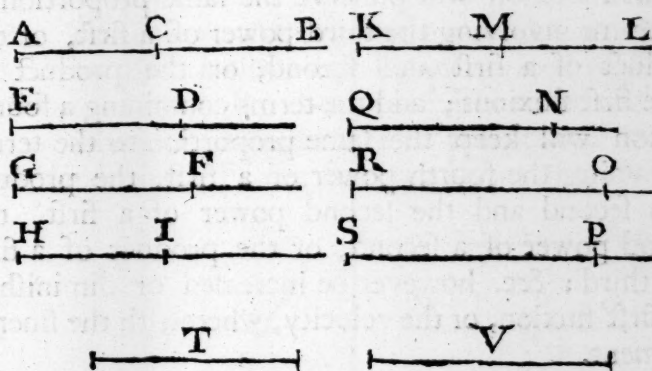
77. Suppose the velocity of the point C be always to the velocity of the point M, as the line T to the line V, when these points are at equal distances from A and K. Then, since the times, in which equal lines are described, are reciprocally as the velocities of the describing points; the time, in which AC receives any additional increment, will be to the time, in which KM shall have received an equal increment, as V to T.

78. Now ED is always to QN in the proportion of T to V. Therefore the variation, by increase or diminution, that ED shall receive to the like variation, which QN shall receive, while the lines AC, KM are augmented by equal increments, will be also as T to V. But the time, wherein ED will receive that variation, to the time, wherein QN will receive its variation, will be as V to T. Consequently, since the velocities, wherewith different lines are described, are as the lines themselves directly, and as the times of description reciprocally, the velocity of the point D to that of the point N will be in the duplicate ratio of T to V.

79. AGAIN, the velocity of D being to the velocity of N, when AC and KM are equal, always in the same duplicate ratio of T to V, and GF being always to RO as the velocity of the point D to the velocity of the point N, the variation, by increase or diminution, of the line GF to the like variation of RO, while AC and KM receive equal augmentation, will also be as the velocity of D to the velocity of N, that is in the duplicate ratio of T to V. But the time, in which the line GF receives its variation, will be to the time, in which RO receives its variation, as V to T. Hence the velocity of the point F
will

will be to the velocity of the point O in the triplicate ratio of T to V.

80. AFTER the same manner, the velocity of the point I will appear to have to the velocity of the point P the quadruplicate of the ratio of T to V.



81. BUT from what we have said above, it is evident, that the velocity of the point D is to the velocity of the point N, as the second fluxion of AC to the second fluxion of KM; the velocity of the point F to the velocity of the point O, as the third fluxion of AC to the third fluxion of KM; and the velocity of the point I to the velocity of the point P, as the fourth fluxion of AC to the fourth fluxion of KM. And hence appears the truth of Sir Isaac Newton's observation at the end of the first proposition of his book of Quadratures, that a second fluxion, and the second power of a first fluxion, or the product under two first fluxions; a third fluxion, and the third power of a first, or the product under a first and second, and so on; are homologous terms in any equation. For, as it appears by this proposition, that if the velocity, wherewith any fluent is augmented, be in any proportion increased; its second fluxion will increase in the duplicate of that

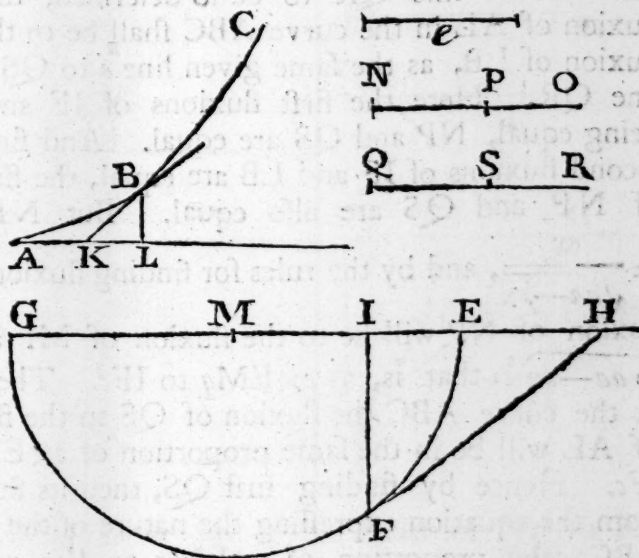
proportion, the third fluxion in the triplicate, and the fourth fluxion in the quadruplicate of that same proportion; it is manifest, that the terms in any equation, that shall involve a second fluxion, will preserve always the same proportion to the terms involving the second power of a first fluxion, or the product of two first fluxions; the terms involving a third fluxion will preserve the same proportion to the terms involving the third power of a first, or the product of a first and second, or the product of three first fluxions; and the terms containing a fourth fluxion will keep the same proportion to the terms containing the fourth power of a first, the product of a second and the second power of a first, the second power of a second, or the product of a first and third; &c. however be increased or diminished the first fluxion, or the velocity, wherewith the fluents augment.

82. IN the problems concerning curve lines, which relate to the degree of curvature in any point of those curves, or to the variation of their curvature in different parts, these superior orders of fluxions are useful; for by the inflexion of the curve, whilst its abscisse flows uniformly, the fluxion of the ordinate must continually vary, and thereby will be attended with these superior orders of fluxions.

83. For example, were it required to compare the different degrees of curvature either of different curves, or of the same curve in different parts, and in order thereto a circle should be sought, whose degree of curvature might be the same with that of any curve proposed, in any point, that should be assigned; such a circle may be found by the help of second fluxions. When the abscisses of two curves flow with equal velocity; where the ordinates have equal first fluxions, the tangents make equal angles

gles with their respective ordinates. If now the second fluxions of these ordinates are also equal, the curves in those points must be equally deflected from their tangents, that is, have equal degrees of curvature. Upon this principle such circles, as have here been mentioned, may be found by the following method.

84. THE curve ABC being given, let it be required to find a circle equally incurvated with this curve at the point B. Suppose EFG to be this circle, in which the tangent FH at the point F makes with the ordinate FI the same angle, as the tangent



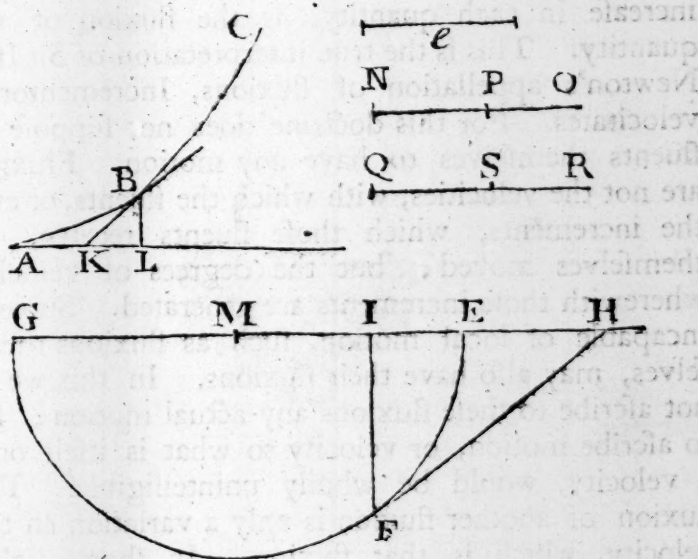
BK, drawn to the other curve ABC at the point B, makes with the ordinate BL of that curve. Now if the two abscisses AL and EI are described with equal velocities, the first fluxions of the ordinates LB and IF will be equal; and therefore, if the two curves are equally incurved at the points B and F, the

the second fluxions of these ordinates will be also equal. If M be the center of the circle EFG , and ME be denoted by a and MI by x , IF will be $=\sqrt{aa-xx}$; and, by the rules for finding fluxions, the first fluxion of IF will be to the fluxion of MI , or of x , as x to $\sqrt{aa-xx}$.

85. Now suppose the line NO to be so described, that the fluxion of MI , or of x , shall be to the first fluxion of IF , as some given line e to NP in the line NO , then will NP be $=\frac{ex}{\sqrt{aa-xx}}$. Suppose likewise the line QR to be so described, that the fluxion of AL in the curve ABC shall be to the first fluxion of LB , as the same given line e to QS in the line QR . Here the first fluxions of IF and LB being equal, NP and QS are equal. And since the second fluxions of IF and LB are equal, the fluxions of NP and QS are also equal. But NP was $=\frac{ex}{\sqrt{aa-xx}}$, and by the rules for finding fluxions, the fluxion of NP will be to the fluxion of MI as ea to $aa-xx^{\frac{3}{2}}$, that is, as $e \times EMq$ to IFc . Therefore in the curve ABC the fluxion of QS to the fluxion of AL will be in the same proportion of $e \times EMq$ to IFc . Hence by finding first QS , then its fluxion, from the equation expressing the nature of the curve ABC , the proportion of $e \times EMq$ to IFc will be given. Consequently the proportion of e to IF will be also given, because the ratio of EMq to IFq is the same with the given ratio of HFq to HIq , or of KBq to KLq . And hereby the circle EFG will be given, whose curvature is equal to the curvature of the curve ABC at the point B .

86. SUPPOSE the curve ABC to be the conical parabola, where ALq shall be equal to $\gamma \times LB$, γ being the latus rectum of the axis. Here e will be to QS as γ to $2AL$; for that is the ratio of the fluxion of AL to the fluxion of BL : therefore QS is

$= \frac{2e}{\gamma} AL$, and consequently the fluxion of QS to the fluxion of AL (that is $e \times EMq$ to IFc) as $2e$ to γ , or as $2e \times EMq$ to $\gamma \times EMq$; in so much that IFc is =



$\frac{1}{2}\gamma \times EMq$, and the given ratio of IFq to EMq (namely the ratio of KLq to KBq) is the same with the ratio of $\frac{1}{2}\gamma$ to IF : that is, IF is equal to half the latus rectum appertaining to the diameter of the parabola, whose vertex is the point B.

87. THIS is all we think necessary towards giving a just and clear idea of the nature of fluxions, and for proving the certainty of the deductions made from

from them. For it must now be manifest to every reader, that mathematical quantities become the proper object of this doctrine of fluxions, whenever they are supposed to increase by any continued motion of prolongation, dilatation, expansion or other kind of augmentation, provided such augmentation be directed by some general rule, whence the measure of the increase of these quantities may from time to time be estimated. And when different homogeneous magnitudes increase after this manner together, one may vary faster than another. Now the velocity of increase in each quantity, is the fluxion of that quantity. This is the true interpretation of Sir Isaac Newton's appellation of fluxions, *Incrementorum velocitates*. For this doctrine does not suppose the fluents themselves to have any motion. Fluxions are not the velocities, with which the fluents, or even the increments, which those fluents receive, are themselves moved; but the degrees of velocity, wherewith those increments are generated. Subjects incapable of local motion, such as fluxions themselves, may also have their fluxions. In this we do not ascribe to these fluxions any actual motion; for to ascribe motion, or velocity to what is itself only a velocity, would be wholly unintelligible. The fluxion of another fluxion is only a variation in the velocity, which is that fluxion. In short, light, heat, sound, the motion of bodies, the power of gravity, and whatever else is capable of variation, and of having that variation assigned, for this reason comes under the present doctrine; nothing more being understood by the fluxion of any subject, than the degree of such its variation.

88. TO assign the velocities of variation or increase in different homogeneous quantities, it is necessary to compare the degrees of augmentation, which

which those quantities receive in equal portions of time; and in this doctrine of fluxions no farther use is made of such increments: for the application of this doctrine to geometrical problems depends upon the knowledge of these velocities only. But the consideration of the increments themselves may be made subservient to the like uses upon other principles; the explanation of which leads us to the second part of our design.



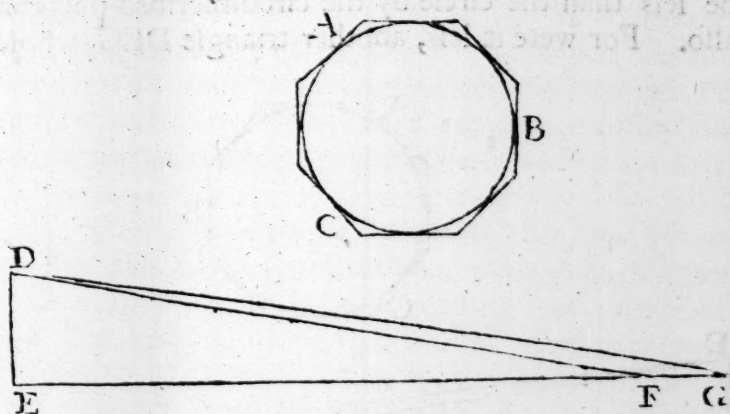
O F

PRIME and ULTIMATE R A T I O S.

89. **T**HE primary method of comparing together the magnitudes of rectilinear spaces is by laying them one upon another: thus all the right lined spaces, which in the first book of Euclide are proved to be equal, are the sum or difference of such spaces, as would cover one another. This method cannot be applied in comparing curvilinear spaces with rectilinear ones; because no part whatever of a curve line can be laid upon a streight line, so as wholly to coincide with it. For this purpose therefore the ancient geometers made use of a method of reasoning, since commonly called the method of exhaustions; which consists in describing upon the curvilinear space a rectilinear one, which though not equal to the other, yet might differ less from it than by any the most minute difference whatever, that should be proposed; and thereby proving, the two spaces, they would compare, could be neither greater nor less than each other.

90. FOR example, in order to prove the equality between the space comprehended within the circumference of a circle, and a triangle, whose base should be equal to the circumference of that circle, and its altitude to the semidiameter, Archimedes takes this method.

method. About the circle he describes a polygon as ABC, and makes it appear, that by multiplying the sides of this polygon, there may at length be described such a one, as shall exceed the circle less than by any difference, that shall be proposed, how minute soever that difference be. By this means it was easy to prove, that the triangle DEF, whose base EF should be equal to the circumference of the

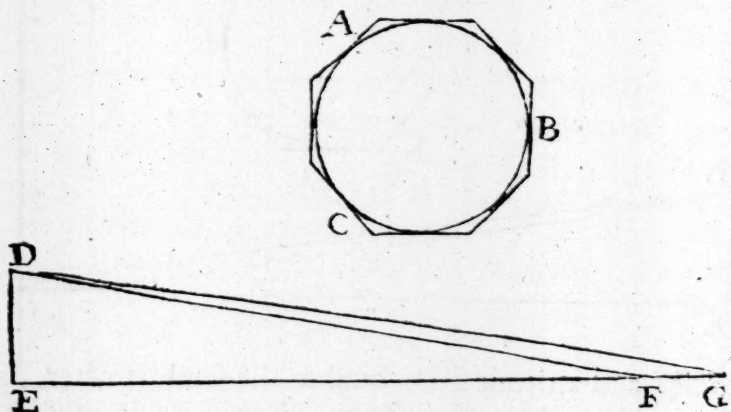


circle, and altitude ED equal to the semidiameter, is not greater than the circle. For were it greater, how small soever be the excess, it were possible to describe about the circle a polygon less than the triangle; but the circumference of the polygon is greater than the circumference of the circle, therefore the polygon can never be less, but must be always greater than the triangle; for the polygon is equal to a triangle, whose altitude is the semidiameter of the circle, and base equal to the circumference of the polygon. It appears therefore impossible for the triangle DEF to be greater than the circle.

91. Thus far Archimedes makes use of the polygon circumscribing the circle and no farther: but inscribing another within the circle he proves, by a similar process of reasoning, that it is impossible for the

the triangle to be less than the circle; because if it were less, a polygon might be inscribed within the circle greater than this triangle, which he also proves to be impossible; whereby at length it becomes certain, that the triangle DEF is neither greater nor less than the circle, but equal to it.

92. HOWEVER, the triangle may be proved not to be less than the circle by the circumscribed polygon also. For were it less, another triangle DEG, whose



base EG is greater than EF, might be taken, which should not be greater than the circle. But a polygon can be circumscribed about the circle, the circumference of which shall exceed the circumference of the circle by less than any line, that can be named, consequently by less than FG, that is, the circumference of the polygon shall be less than EF, and the polygon less than the triangle DEG; therefore it is impossible, that this triangle should not exceed the circle, since it is greater than the polygon: consequently the triangle DEF cannot be less than the circle.

93. THUS the circle and triangle may be proved to be equal by comparing them with one polygon

polygon only, and Sir Isaac Newton has instituted upon this principle a briefer method of conception and expression for demonstrating this sort of propositions, than what was used by the ancients; and his ideas are equally distinct, and adequate to the subject, with theirs, though more complex. It became the first writers to choose the most simple form of expression, and the least compounded ideas possible. But Sir Isaac Newton thought, he should oblige the mathematicians by using brevity, provided he introduced no modes of conception difficult to be comprehended by those, who are not unskilled in the ancient methods of writing.

94. THE concise form, into which Sir Isaac Newton has cast his demonstrations, may very possibly create a difficulty of apprehension in the minds of some unexercised in these subjects. But otherwise his method of demonstrating by the prime and ultimate ratios of varying magnitudes is not only just, and free from any defect in itself; but easily to be comprehended, at least by those who have made these subjects familiar to them by reading the ancients.

95. IN this method any fixed quantity, which some varying quantity, by a continual augmentation or diminution, shall perpetually approach, but never pass, is considered as the quantity, to which the varying quantity will at last or ultimately become equal; provided the varying quantity can be made in its approach to the other to differ from it by less than by any quantity how minute soever, that can be assigned*.

96. UPON this principle the equality between the fore-mentioned circle and triangle DEF is at once

* Princ. Philos. Lib. I. Lem. 1.

deducible. For, since the polygon circumscribing the circle approaches to each according to all the conditions above set down, this polygon is to be considered as ultimately becoming equal to both, and consequently they must be esteemed equal to each other.

97. THAT this is a just conclusion, is most evident. For if either of these magnitudes be supposed less than the other, this polygon could not approach to the least within some assignable distance.

98. RATIOS also may so vary, as to be confined after the same manner to some determined limit, and such limit of any ratio is here considered as that, with which the varying ratio will ultimately coincide *.

99. FROM any ratio's having such a limit, it does not follow, that the variable quantities exhibiting that ratio have any final magnitude, or even limit, which they cannot pass.

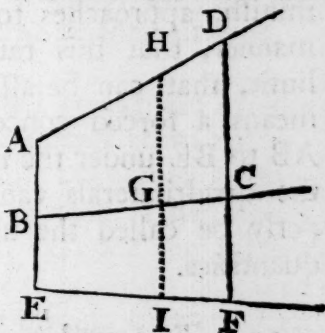
100. For suppose two magnitudes, B and $B+A$, whose difference shall be A , are each of them perpetually increasing by equal degrees. It is evident, that if A remains unchanged, the proportion of $B+A$ to B is a proportion, that tends nearer and nearer to the proportion of equality, as B becomes larger; it is also evident, that the proportion of $B+A$ to B may, by taking B of a sufficient magnitude, be brought at length nearer to the proportion of equality, than to any other assignable proportion; and consequently the ratio of equality is to be considered as the ultimate ratio of $B+A$ to B . The ultimate proportion then of these quantities is

* Princ. Philos. Lib. I. Lem. 1.

here assigned, though the quantities themselves have no final magnitude.

101. THE same holds true in decreasing quantities.

102. THE quadrilateral ABCD bears to the quadrilateral EBCF the proportion of $AB+DC$ to $BE+CF$, provided the two lines AE and DF are parallel. Now if the line DF be drawn nearer to AE, this proportion of $AB+DC$ to $BE+CF$ will not remain the same, unless the lines DA, CB, FE produced will meet in the same point; and this proportion, by diminishing the distance between DF and AE, may at last be brought nearer to the proportion of AB to BE, than to any other whatever. Therefore the proportion of AB to BE is to be considered as the ultimate proportion of $AB+DC$ to $BE+CF$, or as the ultimate proportion of the quadrilateral ABCD to the quadrilateral EBCF.



103. HERE these quadrilaterals can never bear one to the other the proportion between AB and BE, nor have either of them any final magnitude, or even so much as a limit, but by the diminution of the distance between DF and AE they diminish continually without end: and the proportion between AB and BE is for this reason called the ultimate proportion of the two quadrilaterals; because it is the proportion, which those quadrilaterals can never actually have to each other, but the limit of that proportion.

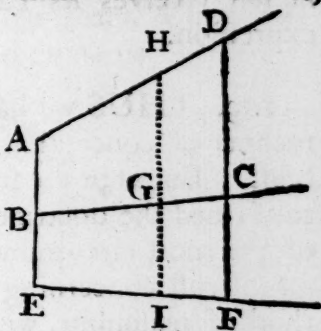
104. THE quadrilaterals may be continually diminished, either by dividing BC in any known proportion

tion in G drawing HGI parallel to AE, by dividing again BG in the like manner, and by continuing this division without end; or else the line DF may be supposed to advance towards AE with an uninterrupted motion, 'till the quadrilaterals quite disappear, or vanish. And under this latter notion these quadrilaterals may very properly be called vanishing quantities, since they are now considered, as never having any stable magnitude, but decreasing by a continued motion, 'till they come to nothing. And since the ratio of the quadrilateral ABCD to the quadrilateral BEFC, while the quadrilaterals diminish, approaches to that of AB to BE in such manner, that this ratio of AB to BE is the nearest limit, that can be assigned to the other; it is by no means a forced conception to consider the ratio of AB to BE under the notion of the ratio, wherewith the quadrilaterals vanish; and this ratio may properly be called the ultimate ratio of two vanishing quantities.

105. THE reader will perceive, I am endeavouring to explain Sir Isaac Newton's expression *Ratio ultima quantitatum evanescentium*; and I have rendered the Latin participle *evanescent*, by the English one *vanishing*, and not by the word *evanescent*, which having the form of a noun adjective, does not so certainly imply that motion, which ought here to be kept carefully in mind. The quadrilaterals ABCD, BEFC become vanishing quantities from the time, we first ascribe to them this perpetual diminution; that is, from that time they are quantities going to vanish. And as during their diminution they have continually different proportions to each other; so the ratio between AB and BE is not to be called merely *Ratio harum quantitatum evanescentium*, but *Ultima ratio* *.

* Vid. Princ. Philos. Lib. I. Lem. xi. in Schol.

106. SHOULD we suppose the line DF first to coincide with the line AE, and then recede from it, by that means giving birth to the quadrilaterals; under this conception the ratio of AB to BE may very justly be considered as the ratio, wherewith the quadrilaterals by this motion commence; and this ratio may also properly be called the first or prime ratio of these quadrilaterals at their origine.



107. HERE I have attempted to explain in like manner the phrase *Ratio prima quantitatum nascentium*; but no English participle occurring to me, whereby to render the word *nascens*, I have been obliged to use circumlocution. Under the present conception of the quadrilaterals they are to be called *nascentes*, not only at the very instant of their first production, but according to the sense, in which such participles are used in common speech, after the same manner, as when we say of a body, which has lain at rest, that it is beginning to move, though it may have been some little time in motion: on this account we must not use the simple expression *Ratio quantitatum nascentium*; for by this we shall not specify any particular ratio; but to denote the ratio between AB and BE we must call it *Ratio prima quadrilaterum nascentium**.

108. WE see here the same ratio may be called sometimes the prime, at other times the ultimate ratio of the same varying quantities, as these quantities are considered either under the notion of vanishing, or

* Vid. Ibid.

of being produced before the imagination by an uninterrupted motion. The doctrine under examination receives its name from both these ways of expression.

109. **THUS** we have given a general idea of the manner of conception, upon which this doctrine is built. But as in the former part of this discourse we confirmed the doctrine of fluxions by demonstrations of the most circumstantial kind; so here, to remove all distrust concerning the conclusiveness of this method of reasoning, we shall draw out its first principles into a more diffusive form.

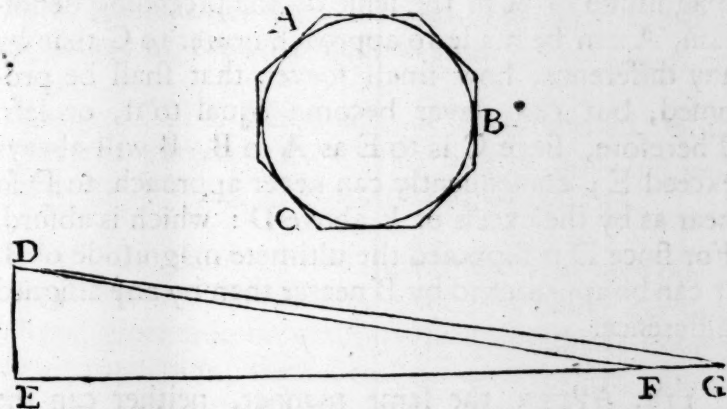
110. **FOR** this purpose, we shall in the first place define an ultimate magnitude to be the limit, to which a varying magnitude can approach within any degree of nearness whatever, though it can never be made absolutely equal to it.

111. **THUS** the circle discoursed of above, according to this definition, is to be called the ultimate magnitude of the polygon circumscribing it; because this polygon, by increasing the number of its sides, can be made to differ from the circle, less than by any space, that can be proposed, how small soever; and yet the polygon can never become either equal to the circle or less.

112. **IN** like manner the circle will be the ultimate magnitude of the polygon inscribed, with this difference only, that as in the first case the varying magnitude is always greater, here it will be less than the ultimate magnitude, which is its limit.

113. **AGAIN**, if EG, the base of the triangle DEG, be supposed always equal to the circumference of the polygon, the triangle DEF will be the ultimate

imate magnitude of the triangle DEG, since the base EG will constantly be greater than the base EF, equal to the circumference of the circle only, and yet EG may be made to approach EF nearer than by any difference, that can be named.



114. UPON this definition we may ground the following proposition; That, when varying magnitudes keep constantly the same proportion to each other, their ultimate magnitudes are in the same proportion.

115. LET A and B be two varying magnitudes, which keep constantly in the same proportion to each other; and let C be the ultimate magnitude of A, and D the ultimate magnitude of B. I say, that C is to D in the same proportion.

116. As A is a varying magnitude continually approaching to C, but can never become equal to it, A may be either always greater or always less than C. In the first place suppose it greater. When A is greater than C, in approaching to C it is continually diminished; therefore B keeping always in the same proportion to A, B in approaching to its limit D is also continually diminished.

A. B.
C. D.
E.

D 4 117. Now,

117. Now, if possible, let the ratio of C to D be greater than that of A to B, that is, suppose C to have to some magnitude E, greater than D, the same proportion as A has to B. Since C is the ultimate magnitude of A in the sense of the preceding definition, A can be made to approach nearer to C than by any difference, how small soever that shall be proposed, but can never become equal to it, or less. Therefore, since C is to E as A to B, B will always exceed E; consequently can never approach to D so near as by the excess of E above D: which is absurd. For since D is supposed the ultimate magnitude of B, it can be approached by B nearer than by any assigned difference.

118. AFTER the same manner, neither can the ratio of D to C be greater than that of B to A.

119. If the varying magnitude A be less than C, it follows, in like manner, that neither the ratio of C to D can be less than that of A to B, nor the ratio of D to C less than that of B to A,

120. IT is an evident corollary from this proposition, that the ultimate magnitudes of the same or equal varying magnitudes are equal.

121. NOW from this proposition the fore-mentioned equality between the circle and triangle DEF will again readily appear. For the circle being the ultimate magnitude of the polygon, and the triangle DEF the ultimate magnitude of the triangle DEG, when the polygon and the triangle DEG are equal, by this proposition the circle and triangle DEF will be also equal.

122. If

122. IF the preceding proposition be admitted, as a genuine deduction from the definition, upon which it is grounded; this demonstration of the equality of the circle and triangle cannot be excepted to. For no objection can lie against the definition itself, as no inference is there deduced, but only the sense explained of the term defined in it.

123. THE other part of this method, which concerns varying ratios, may be put into the same form by defining ultimate ratios, as follows.

124. IF there be two quantities, that are (one or both) continually varying, either by being continually augmented, or continually diminished; though the proportion, they bear to each other, should by this means perpetually vary, but in such a manner, that it constantly approaches nearer and nearer to some determined proportion, and can also be brought at length in its approach nearer to this determined proportion than to any other, that can be assigned, but can never pass it: this determined proportion is then called the ultimate proportion, or the ultimate ratio of those varying quantities.

125. TO this definition of the sense, in which the term ultimate ratio, or ultimate proportion is to be understood, we must subjoin the following proposition: That all the ultimate ratios of the same varying ratio are the same with each other.

126. SUPPOSE the ratio of A to B continually varies by the variation of one or both of the terms A and B. If the ratio of C to D be the ultimate ratio of A to B, and the ratio of E to F be likewise the ultimate ratio of the same; I say, the ratio of C to D is the same with the ratio of E to F.

127. If possible, let the ratio of E to F differ from that of C to D. Since the ratio of C to D is the ultimate ratio of A to B, the ratio of A to B, in its approach to that of C to D, can be brought at length nearer to it, than to any other whatever. Therefore if the ratio of E to F differ from that of C to D, the ratio of A to B will either pass that of E to F, or can never approach so near it, as to the ratio of C to D: in so much that the ratio of E to F cannot be the ultimate ratio of A to B, in the sense of this definition.

128. THE two definitions here set down, together with the general propositions annexed to them, comprehend the whole foundation of this method, we are now explaining.

129. WE find in former writers some attempts toward so much of this method, as depends upon the first definition. Lucas Valerius in a most excellent treatise on the center of gravity of solid bodies, has given a proposition nothing different, but in the form of the expression, from that we have subjoined to our first definition; from which he demonstrates with brevity and elegance his propositions concerning the mensuration and center of gravity of the sphere, spheroid, parabolical and hyperbolical conoids. This author writ before the doctrine of indivisibles was proposed to the world. And since, Andrew Tacquet, in his treatise on the cylindrical and annular solids, has made the same proposition, though something differently expressed, the basis of his demonstrations, at the same time very judiciously exposing the inconclusiveness of the reasonings from indivisibles. However, the consideration of the limits of varying proportions, when the quantities expressing those proportions have themselves no limits, which

which renders this method of prime and ultimate ratios much more extensive, we owe intirely to Sir Isaac Newton. That this method, as thus compleated, is applicable not only to the subjects treated by the ancients in the method of exhaustions, but to many others also of the greatest importance, appears from our author's immortal treatise on the Mathematical principles of natural philosophy.

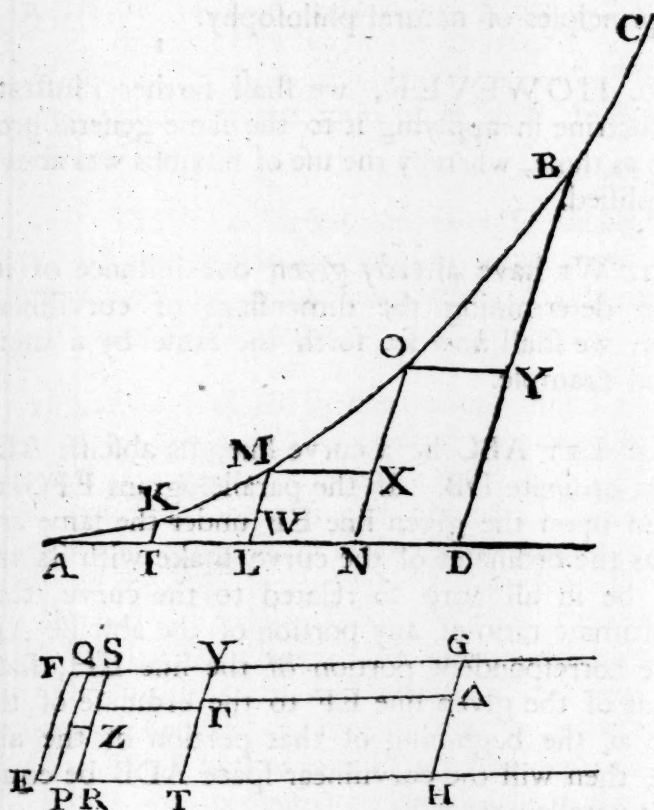
130. HOWEVER, we shall farther illustrate this doctrine in applying it to the same general problems as those, whereby the use of fluxions was above exemplified.

131. WE have already given one instance of its use in determining the dimensions of curvilinear spaces; we shall now set forth the same by a more general example.

132. LET ABC be a curve line, its abscisse AD, and an ordinate DB. If the parallelogram EFGH, formed upon the given line EF under the same angle, as the ordinates of the curve make with its abscisse, be in all parts so related to the curve, that the ultimate ratio of any portion of the abscisse AD to the correspondent portion of the line EH, shall be that of the given line EF to the ordinate of the curve at the beginning of that portion of the abscisse; then will the curvilinear space ADB be equal to the parallelogram EG.

133. IN the curve let the abscisse AD be divided into any number of equal parts AI, IL, LN, ND, and let the ordinates IK, LM, NO be drawn, and also in the parallelogram EG the correspondent lines PQ, RS and TV. In the curve compleat the parallelograms IW, LX, NY, and in the parallelogram EG make the parallelogram PZ equal to the paral-

parallelogram IW , the parallelogram $R\Gamma$ equal to LX , and the parallelogram $T\Delta$ equal to NY : then the whole figure $IKWMOYD$ will be equal to the whole figure $PZ\Gamma\Delta H$. But in the curve, by increasing the number and diminishing the breadth of these parallelograms, the figure $IKWMOYD$



will approach nearer and nearer in magnitude to the curvilinear space ADB ; in so much that their difference may be reduced to less than any space, that shall be assigned; therefore the curvilinear space ADB is the ultimate magnitude of the figure $IKWMOYD$. Farther, since the parallelogram EG is in all parts so related to the curve, that the ultimate ratio of every por-

portion, as LN, of the abscisse AD to RT, the correspondent portion of EH, is the same with the ratio of EF or RS, to LM; the ultimate ratio of the parallelogram LX, or its equal RF, to the parallelogram RV, is the ratio of equality. This is also true of all the other correspondent parallelograms; therefore, the ultimate ratio of the figure PZΓΔH to the parallelogram PG is the ratio of equality; that is, the figure PZΓΔH, by increasing the number of its parallelograms, can be brought nearer to the parallelogram PG than by any difference whatever, that may be proposed. Moreover, by increasing the number of ordinates in the curve, the residuary portion AI of the abscisse can be reduced to less than any magnitude, that shall be proposed; whereby the parallelogram EQ, corresponding to this portion of the abscisse, may be also reduced to less than any magnitude, whatever proposed; and the parallelogram PG be brought to differ less from EG than by any assigned magnitude how small soever. Since therefore the figure PZΓΔH can be brought nearer to the parallelogram PG than by any difference, that can be assigned; the figure PZΓΔH can be brought also nearer to the parallelogram EG than by any difference that can be assigned. Consequently the parallelogram EG is the ultimate magnitude of the figure PZΓΔH. Therefore the figures PZΓΔH and IKWMXOYD being equal varying magnitudes, and the ultimate magnitudes of equal varying magnitudes being equal, the curvilinear space ADB is equal to the parallelogram EG.

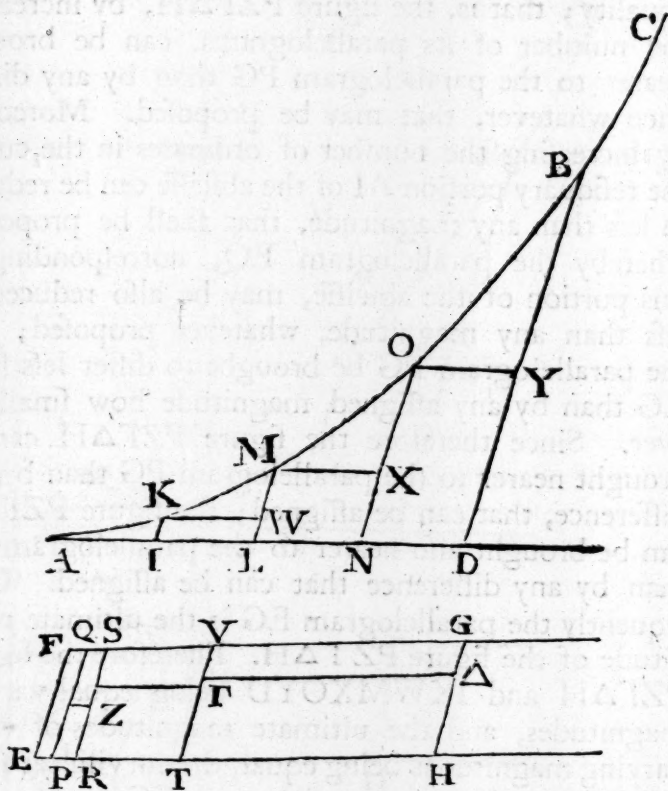
134. SUPPOSE the curve ABC were a cubical parabola convex to the abscisse, that is, suppose $\odot$ a given line, and $\odot q \times LM = ALc$. If EH be $= \frac{ADq}{4\odot q \times EF}$, then the parallelogram EG will be equal to the space ADB.

As

135. As EH is $= \frac{ADqq}{4\Theta q \times EF}$, ER will be $= \frac{ALqq}{4\Theta q \times EF}$
 and $ET = \frac{ANqq}{4\Theta q \times EF}$, consequently $RT =$

$$\frac{ALc \times LN + \frac{3}{2}ALq \times LNq + AL \times Lnc + \frac{1}{4}LNqq}{\Theta q \times EF}$$

Therefore the parallelogram EG is here so related in

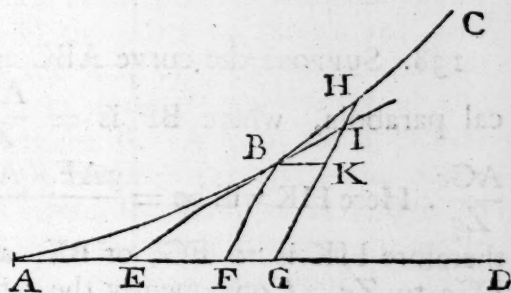


all parts to the curve, that LN is to RT as $\Theta q \times EF$ to $ALc + \frac{3}{2}ALq \times LN + AL \times Lnc + \frac{1}{4}LNc$. Now it is evident, that the ratio of LN to RT can never be so great as the ratio of $\Theta q \times EF$ to ALc ; but yet, by diminishing LN , the ratio of LN to RT may at length be brought nearer to this ratio than

to any other whatever, that should be proposed. Consequently by the preceding definition of what is to be understood by an ultimate ratio, the ratio of $\Theta q \times EF$ to ALc is the ultimate ratio of LN to RT . But ALc being $= \Theta q \times LM$, $\Theta q \times EF$ is to ALc as EF to LM . Therefore the ratio of EF to LM is the ultimate ratio of LN to RT . Consequently, by the preceding general proposition, the parallelogram EG is equal to the curvilinear space ADB . And this parallelogram is equal to $\frac{1}{2}AD \times DB$.

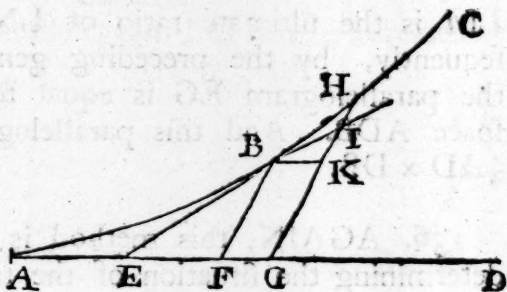
136. AGAIN, this method is equally useful in determining the situation of the tangents to curve lines.

137. IN the curve ABC , whose abscisse is AD , let EB be a tangent at the point B . Let BF be the ordinate at the same point B , and GH another ordinate parallel to it, which shall meet the tangent in I , and the line BK , parallel to the abscisse AD , in K . Here the ratio of HK , the difference of the ordinates, to BK can never be the same with the ratio of BF to FE , unless by the figure of the curve the tangent chance to cut it in some point remote from B ; this ratio of BF to FE being the same with that of IK to KB . But it is farther evident, that the nearer GH is to FB , the ratio of KH to KB will approach so much the nearer to the ratio of IK to KB ; and the angle, which the curve BC makes with the



tangent

tangent BI being less than any right-lined angle, it is manifest, that GH may be made to approach towards FB, 'till the ratio of HK to KB, shall at length approach nearer to the ratio of IK to KB, or of BF to FE, than to any other ratio whatever, that shall be proposed; that is, the ratio of BF to FE is the ultimate ratio of HK to KB. There-



fore, if from the properties of the curve ABC the ratio of HK to KB be determined, and from thence their ultimate ratio assigned; this ratio thus assigned will be the ratio of BF to FE; because all the ultimate ratios of the same variable ratio are the same with each other.

138. SUPPOSE the curve ABC again to be a cubical parabola, where BF is $= \frac{AFc}{Zq}$, and GH $= \frac{AGc}{Zq}$. Here HK will be $= \frac{3AF \times AG \times FG + FGc}{Zq}$; therefore HK is to FG, or BK, as $3AF \times AG + FGq$ to Zq . Consequently the ratio of HK to BK can never be so small as the ratio of $3AFq$ to Zq ; but by diminishing BK it may be brought nearer to that ratio than to any other determinate ratio whatever; that is, the ratio of $3AFq$ to Zq is the ultimate ratio of HK to KB. Therefore, if BF bear to FE the ratio of $3AFq$ to Zq , the line BE will touch the curve in B: and EF will be equal to $\frac{1}{3}AF$.

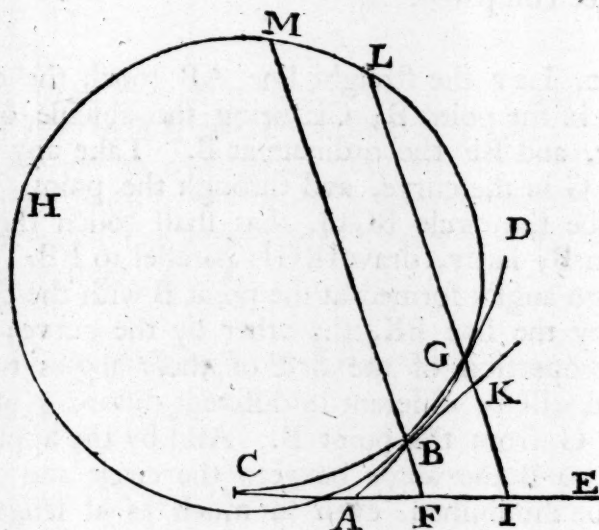
139. AFTER the situation of the tangent has been thus determined, the magnitude of HI, the part of the ordinate intercepted between the tangent and the curve, will be known. For example, in this instance since BF is to FE, that is IK to FG, as $3 AFq$ to Zq , IK will be $= \frac{3 AFq \times FG}{Zq}$, and

$$HK \text{ being } = \frac{3 AF \times AG \times FG + FGc}{Zq}, \text{ HI will be } = \frac{3 AF \times FGq + FGc}{Zq} = \frac{FGq}{Zq} \times \frac{3 AF + FG}{1}.$$

Now by this line HI may the curvature of curve lines be compared.

140. LET the streight line AB touch the curve CBD in the point B; CE being the abscisse of the curve, and BF the ordinate at B. Take any other point G in the curve, and through the points G, B, describe the circle BGH, that shall touch the line AB in B; lastly, draw IKGL parallel to FB. Here are two angles formed at the point B with the circle, one by the line BK, the other by the curve; and the proportion of the first of these angles to the second will be different in different distances of the point G from the point B. And by the approach of G to B the angle between the circle and curve will be diminished, even so much as at length to bear a less proportion to the angle between the circle and tangent, than any, that can be proposed. That is, by the approach of the point G to B the angle between the tangent and circle may be brought nearer to the angle between the tangent and the curve, than by any difference how minute soever homogeneous to those angles; therefore the magnitude of the circle being continually varied by the gradual approach of G to B, and the angle between the tangent and circle thereby also varied; the angle

between the tangent and curve is the ultimate magnitude of these angles. That is, the ultimate of these circles determines the degree of curvature of the curve CBD at the point B. But in the circle the rectangle under LKG is equal to the square of BK. And while the magnitude of KL varies perpetually by the approach of the point G towards B; if BM, taken in FB produced, be the ultimate magnitude of KL, the circle described through M and B to touch the tangent AK in B will be the circle, by which the curvature of the curve CBD in B is to be estimated.



141. SUPPOSE the curve CBD to be the cubical parabola as before, where $Zq \times FB$ is $= CFc$, then KG will be $= \frac{FIq}{Zq} \times \sqrt[3]{CF + FI}$. Hence LK ($= \frac{BKq}{KG}$) is $= \frac{BKq}{FIq} \times \frac{Zq}{\sqrt[3]{CF + FI}}$. But it is evident, that in a given situation of the tangent AB the ratio of BKq to FIq is given; therefore LK

LK will be reciprocally as $3CF + FI$, and will continually increase, as the point G approaches to the point B, but can never be so great, as to equal $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$; yet by the near approach of G to B, LK may be brought nearer to this quantity than by any difference, that should be proposed. Therefore, by our former definition of ultimate magnitudes, $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$ is the ultimate magnitude of LK. Consequently, if BM be taken equal to this $\frac{BKq}{FIq} \times \frac{Zq}{3CF}$, the circle described through M is that required.

142. WE have now gone through all, we think needful for illustrating the doctrine of prime and ultimate ratios; and by the definitions, which have been here given of ultimate magnitudes and proportions, compared with the instances subjoined of the application of this doctrine to geometrical problems, we hope our readers cannot fail of forming so distinct a conception of this method of reasoning, that it shall appear to them equally geometrical and scientific with the most unexceptionable demonstration.

143. THEREFORE we shall in the next place proceed to consider the demonstrations, which Sir Isaac Newton has himself given, upon the principles of this method, of his precepts for assigning the fluxions of flowing quantities.

O F

Sir ISAAC NEWTON'S.

M E T H O D

Of demonſtrating his Rules for finding

F L U X I O N S.

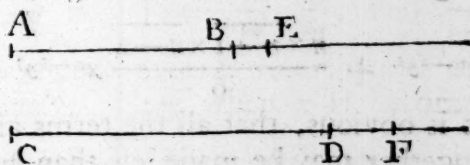
144. **S**IR Isaac Newton has comprehended his directions for computing the fluxions of quantities in two propositions; one in his Introduction to his treatise on the Quadrature of curves; the other is the first proposition of the book itself. In the first he assigns the fluxion of a simple power, the latter is universal for all quantities whatever.

145. **F**OR determining the fluxion of a simple power suppose the line AB to be denoted by x , and another line CD to be denoted by $\frac{x^n}{a^{n-1}}$, or by considering a as unite, CD will be denoted by x^n .

146. **S**UPPOSE the points B and D to move in equal spaces of time into two other positions E and F; then DF will be to BE in the ratio of the velocity, wherewith DF would be described with a uniform motion,

motion, to the velocity, wherewith BE will be described in the same time with a uniform motion.

But if the point describing the line AB moves uniformly; the velocity, wherewith the line CD is described,



will not be uniform. Therefore the space DF is not described with a uniform velocity; in so much that the velocity, wherewith DF would be uniformly described, is never the same with the velocity at the point D. But by diminishing the magnitude of DF, the uniform velocity, wherewith DF would be described, may be made to approach at pleasure to the velocity at the point D. Therefore the velocity at the point D is the ultimate magnitude of the velocity, wherewith DF would be uniformly described. Consequently the ratio of the velocity at D to the velocity at B is the ultimate ratio of the velocity, wherewith DF would be uniformly described, to the velocity, wherewith BE is uniformly described. But DF being to BE as the velocity, wherewith DF would be uniformly described, to that, wherewith BE is uniformly described, the ultimate ratio of DF to BE is also the ultimate ratio of the first of these velocities to the last; because all the ultimate ratios of the same varying ratio are the same with each other. Therefore the ratio of the velocity at D to the velocity at B, that is, of the fluxion of CD to the fluxion of AB, is the same with the ultimate ratio of DF to BE.

147. IF now the augment BE be denoted by o , the augment DF will be denoted by $nx^{n-1}o + \frac{n \times n-1}{2} x^{n-2}o^2 + \frac{n \times n-1 \times n-2}{6} x^{n-3}o^3 + \&c.$ And here

it is obvious, that all the terms after the first taken together may be made less than any part whatever of the first, that shall be assigned. Consequently the proportion of the first term $nx^{n-1}o$ to the whole augment may be made to approach within any degree whatever of the proportion of equality; and therefore the ultimate proportion of $nx^{n-1}o + \frac{n \times n-1}{2} x^{n-2}o^2 + \frac{n \times n-1 \times n-2}{6} x^{n-3}o^3 + \&c.$ to o , or of DF to BE, is that of $nx^{n-1}o$ only to o , or the proportion of nx^{n-1} to 1.

148. AND it has already been proved, that the proportion of the velocity at D to the velocity at B is the same with the ultimate proportion of DF to BE; therefore the velocity at D is to the velocity at B, or the fluxion of x^n to the fluxion of x , as nx^{n-1} to 1.

149. IN the first proposition of the treatise of Quadratures the author proposes the relation betwixt three varying quantities x , y , and z to be expressed by this equation $x^3 - xy^2 + a^2z - b^3 = 0$. Suppose these quantities to be augmented by any contemporaneous increments great or small. Let us also suppose some quantity o to be described at the same time by some known velocity, and let that velocity be denoted by m ; the velocity, wherewith the augment of x would be uniformly described in that time be denoted by $\dot{x}$; the velocity, wherewith the augment of y would be uniformly described in the

the same time by $\dot{y}$; and lastly the velocity, where-
with the augment of z would be uniformly described
in the same time by $\dot{z}$. Then $\frac{o\dot{x}}{m}$, $\frac{o\dot{y}}{m}$, and $\frac{o\dot{z}}{m}$

will express the contemporaneous increments of
 x , y , and z respectively. Now when x is become

$x + \frac{o\dot{x}}{m}$, y is become $y + \frac{o\dot{y}}{m}$ and z become $z +$

$\frac{o\dot{z}}{m}$; the former equation will become $x^3 + \frac{3x^2o\dot{x}}{m}$

$+ \frac{3xo^2\dot{x}\dot{x}}{m^2} + \frac{o^3\dot{x}^3}{m^3} - xy^2 - \frac{o\dot{x}y^2}{m} - \frac{2xo\dot{y}y}{m}$

$- \frac{2\dot{x}o^2\dot{y}y}{m^2} - \frac{xo^2\dot{y}\dot{y}}{m^2} - \frac{\dot{x}o^3\dot{y}\dot{y}}{m^3} + a^2z + \frac{a^2o\dot{z}}{m} -$

$b^3 = 0$. Here, as the first of these equations exhi-
bits the relation between the three quantities x , y , z ,

as far as the same can be expressed by a single equa-
tion; so this second equation, with the assistance of

the first, will express the relation between the aug-
ments of these quantities. But the first of these equa-

tions may be taken out of the latter; whence will
arise this third equation $\frac{3x^2o\dot{x}}{m} + \frac{3xo^2\dot{x}\dot{x}}{m^2} +$

$\frac{o^3\dot{x}^3}{m^3} - \frac{o\dot{x}y^2}{m} - \frac{2xo\dot{y}y}{m} - \frac{2\dot{x}o^2\dot{y}y}{m^2} - \frac{xo^2\dot{y}\dot{y}}{m^2}$

$- \frac{\dot{x}o^3\dot{y}\dot{y}}{m^3} + \frac{a^2o\dot{z}}{m} = 0$; which also expresses the

relation between the several increments; and like-
wise if o be a given quantity, this equation will

equally express the relation between the velocities,
wherewith these several increments are generated

respectively by a uniform motion. And this equa-
tion being divided by o will be reduced to more

simple terms, and yet will equally express the rela-
tion of these velocities; and then the equation will

become $\frac{3x^2\dot{x}}{m} + \frac{3xo\dot{x}\dot{x}}{m^2} + \frac{o^2\dot{x}^2}{m^3} - \frac{\dot{x}y^2}{m} -$
 $\frac{2x\ddot{y}y}{m} - \frac{2\dot{x}o\ddot{y}y}{m^2} - \frac{x\ddot{o}\ddot{y}y}{m^2} - \frac{\dot{x}o^2\ddot{y}y}{m^3} + \frac{a^2\dot{z}}{m} = 0.$

Now let us form an equation out of the terms of this, from which the quantity o is absent. This equation will be $\frac{3x^2\dot{x}}{m} - \frac{\dot{x}y^2}{m} - \frac{2x\ddot{y}y}{m} + \frac{a^2\dot{z}}{m} = 0;$ and this equation multiplied by m becomes $3x^2\dot{x} - \dot{x}y^2 - 2x\ddot{y}y + a^2\dot{z} = 0.$ It is evident, that this equation does not express the relation of the fore-mentioned velocities; yet by the diminution of o this equation may come within any degree of expressing that relation. Therefore, by what has been so often inculcated, this equation will express the ultimate relation of these velocities. But the fluxions of the quantities x, y, z are the ultimate magnitudes of these velocities; so that the ultimate relation of these velocities is the relation of the fluxions of these quantities. Consequently this last equation represents the relation of the fluxions of the quantities $x, y, z.$

150. IT is now presumed, we have removed all difficulty from the demonstrations, which Sir Isaac Newton has himself given, of his rules for finding fluxions.

151. IN the beginning of this discourse we have endeavoured at such a description of fluxions, as might not fail of giving a distinct and clear conception of them. We then confirmed the fundamental rules for comparing fluxions together by demonstrations of the most formal and unexceptionable kind. And now having justified Sir Isaac Newton's own demonstrations, we have not only shewn, that his doctrine of fluxions is an unerring guide in the solution

lution of geometrical problems, but also that he himself had fully proved the certainty of this method. For accomplishing this last part of our undertaking it was necessary to explain at large another method of reasoning established by him, no less worthy consideration; since as the first inabled him to investigate the geometrical problems, whereby he was conducted in those remote searches into nature, which have been the subject of universal admiration, so to the latter method is owing the surprizing brevity, wherewith he has demonstrated those discoveries.



CONCLU-

CONCLUSION.

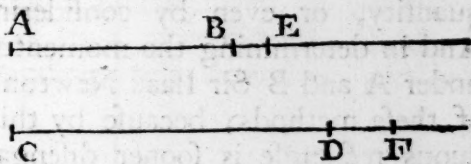
152. **T**HUS we have at length finished the whole of our design, and shall therefore put a period to this discourse with the explanation of the term momentum frequently used by Sir Isaac Newton, though we have yet had no occasion to mention it.

153. **A**ND in this I shall be the more particular, because Sir Isaac Newton's definition of momenta, That they are the momentaneous increments or decrements of varying quantities, may possibly be thought obscure. Therefore I shall give a fuller delineation of them, and such a one, as shall agree to the general sense of his description, and never fail to make the use of this term, in every proposition, where it occurs, clearly to be understood.

154. **I**N determining the ultimate ratios between the contemporaneous differences of quantities, it is often previously required to consider each of these differences apart, in order to discover, how much of those differences is necessary for expressing that ultimate ratio. In this case Sir Isaac Newton distinguishes, by the name of momentum, so much of any difference,

ence, as constitutes the term used in expressing this ultimate ratio.

155. Thus in § 147, where BE is = 0, and DF equal to $nx^{n-1}o + \frac{n \times n-1}{2} x^{n-2}o^2 + \frac{n \times n-1 \times n-2}{6} x^{n-3}o^3 + \&c.$ the ultimate ratio of DF to BE being the ratio of $nx^{n-1}o$ to 0, such a part only of DF as is denoted by $nx^{n-1}o$, without the addition of any of the following terms of the series, constitutes the whole of the momentum of the line CD; but the momentum of AB is the same as the whole difference BE, or 0.



156. In like manner, if A and B denote varying quantities, and their contemporaneous increments be represented by a and b ; the rectangle under any given line M and a is the contemporaneous increment of the rectangle under M and A, and $A \times b + B \times a + a \times b$ is the like increment of the rectangle under A, B. And here the whole increment $M \times a$ represents the momentum of the rectangle under M, A; but $A \times b + B \times a$ only, and not the whole increment $A \times b + B \times a + a \times b$, is called the momentum of the rectangle under A, B; because so much only of this latter increment is required for determining the ultimate ratio of the increment of $M \times A$ to the increment of $A \times B$, this ratio being the same with the ultimate ratio of $M \times a$ to $A \times b + B \times a$; for the ultimate ratio of $A \times b + B \times a$ to $A \times b + B \times a + a \times b$ is the ratio of equality. Consequently the ultimate ratio of $M \times a$

$M \times a$ to $A \times b + B \times a$ differs not from the ultimate ratio of $M \times a$ to $A \times b + B \times a + a \times b$.

157. THESE momenta equally relate to the decrements of quantities, as to their increments, and the ultimate ratio of increments, and of decrements at the same place is the same; therefore the momentum of any quantity may be determined, either by considering the increment, or the decrement of that quantity, or even by considering both together. And in determining the momentum of the rectangle under A and B Sir Isaac Newton has taken the last of these methods; because by this means the superfluous rectangle is sooner disengaged from the demonstration.

158. HERE it must always be remembered, that the only use, which ought ever to be made of these momenta, is to compare them one with another, and for no other purpose than to determine the ultimate or prime proportion between the several increments or decrements, from whence they are deduced*. Herein the method of prime and ultimate ratios essentially differs from that of indivisibles; for in the method of indivisibles momenta are considered absolutely as parts, whereof their respective quantities are actually composed. But though these momenta have no final magnitude, which would be necessary to make them parts capable of compounding a whole by accumulation; yet their ultimate ratios are as truly assignable as the ratios between any quantities whatever. Therefore none of the objections made against the doctrine of indivisibles are of the least weight against this method: but while we attend carefully to the observation here laid down, we shall be as secure against error, and the mind will receive as full satisfaction,

* Neque spectatur magnitudo momentorum, sed prima nascentium proportio. Newt. Princ. Phil. Lib. II. Lem. 2.

as in any the most unexceptionable demonstration of Euclide.

159. WE shall make no apology for the length of this discourse: for as we can scarce suspect, after what has been above written, that our readers will be at any loss to remove of themselves, whatever little difficulties may have arisen in this subject from the brevity of Sir Isaac Newton's expressions; so our time cannot be thought misemployed, if we shall at all have contributed, by a more diffusive phrase, to the easier understanding these extensive, and celebrated inventions.



AN
ACCOUNT
OF THE
PRECEDING DISCOURSE.

*First printed in The Present State of the Republick
of Letters for October 1735.*

1. **S**OME doubts having lately arisen concerning Sir *Isaac Newton's* doctrines of fluxions, and of prime and ultimate ratios; this treatise was written with design to give such an idea of both these subjects, as might clear them from uncertainty, without entering into the discussion of any particular objections.

2. For this end the author has been careful, not only to distinguish both these methods from that usually known by the name of indivisibles, but also from each other.

3. The manner wherein the ancients demonstrated, what relates to the mensuration of curvilinear spaces, not giving any distinct notion of the principles, upon which they built their analysis of such problems; about thirty years before Sir *Isaac Newton* invented his method of fluxions, *Cavalierius*, a mathematician of *Italy*, proposed in these problems a new form of reasoning*. He supposes, that all surfaces might

* In *Geometria Indivisibilibus* promota, Edit. 1635.

be filled up with parallel lines, and all solids by parallel planes; and then lays down this fundamental proposition, That different planes are in the same proportion to each other as all the lines contained in each, and different solids in the same proportion as all the planes contained in them. As this is a manner of expression hardly accompanied with any ideas; since it is not at all intelligible to speak of the collective number of lines or planes, where their number is wholly undetermined and infinite: this method was from the beginning opposed, as ungeometrical, and in no measure agreeable to that clearness of conception and expression, for which the mathematical sciences had ever been celebrated. But however, as by proper cautions error in the conclusions might be avoided, and this method promised great assistance in the analysis of a subject, wherein the ancients had made the least progress; it was warmly espoused by *Torricellius*, and others of the most illustrious geometers. Our countryman Dr. *Wallis*, and many after him, thought it an improvement of this method to substitute, in the room of lines, parallelograms, whose breadth was to be called infinitely small; and for the planes, whereby solids were supposed to be filled up, prisms or cylinders of an infinitely small altitude: so that the method of indivisibles at length supposed all geometrical magnitudes, whether lines, surfaces, or solids, to be composed of an infinite number of homogeneous magnitudes, each infinitely small.

4. But this is a mode of expression no way more intelligible than the other. Sir *Isaac Newton* therefore instituted a manner of conception upon quite different principles. He observing (to use his own words) that indivisibles have no being either in geometry or in nature*; instead of this infinite and in-

* *Philos. Transact.* No. 342. p. 205. or *Commer. Epistolic.* p. 38.

conceivable subdivision of magnitudes already formed, he considered them as produced before the imagination by some motion. And thus the same magnitude will in some parts increase faster than in others, and different magnitudes described together increase by different degrees of swiftness. Now if the proportion between the celerity of increase of two magnitudes produced together is in all parts known; it is evident, that the relation between the magnitudes themselves must from thence be discoverable.

5. This is the foundation of his method of fluxions. And to form a true idea thereof, we must distinguish between the increase, which a line or figure receives in any given space of time, and the velocity, wherewith that increase is generated: for though by the velocity, wherewith any line or figure continually augments, the quantity of its increase may be known; and on the contrary, from the quantity of the increase the velocity, wherewith that increase was produced; yet the quantity thus added is not the velocity, wherewith it is generated. And the method of fluxions requires the knowledge of these velocities of increase only: as Sir *Isaac Newton's* other method of prime and ultimate ratios proceeds entirely upon the consideration of the increments produced.

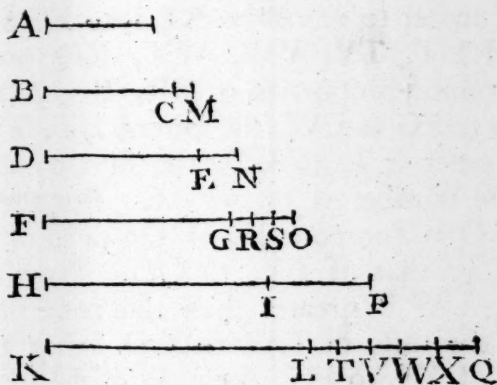
6. In the method of fluxions are introduced no forms of expression, but what convey very clear and distinct ideas, and such as have not the least affinity with the mode of conception schemed out in that of indivisibles. This manner therefore of considering magnitudes, as they are gradually produced before the mind, is a genuine way of discovering the relation between such magnitudes, which may afterwards be proved to bear that relation by a subdivision into parts, as practised by the ancient geometers; since it is shewn in this discourse, that the rules for
finding

THE PRECEDING DISCOURSE. 81

finding fluxions are demonstrable according to the ancient forms.

7. The demonstration here given for the fluxion of a power, is formed upon the model of that delivered by Sir *Isaac Newton* himself; but may be otherwise performed after the following manner.

8. Let A, BC, DE, FG, HI, KL, and A, BM, DN, FO, HP, KQ be two series's of continued proportionals beginning with the same term A; then if b be a number denoting the distance of the terms KL, KQ from A, and k a number denoting the



distance of the terms FG, FO from the same; the ratio of LQ, the difference of the terms KL, KQ the most remote from A, to GO, the difference of the other terms FG, FO, is greater than the ratio of $b \times KL$ to $k \times FG$, and less than the ratio of $b \times KQ$ to $k \times FO$.

9. The ratio of DE to DN is the duplicate of the ratio of BC to BM; for the ratio of DE to A is the duplicate of that of BC to A, and the ratio of A to DN is the duplicate of that of A to BM; therefore, by equality, the ratio of DE to DN is the duplicate of the ratio of BC to BM.

10. In like manner the ratio of FG to FO is the triplicate of the ratio of BC to BM, and so of the rest. Inasmuch that between any two terms in these series's, equally distant from the first term A, as many mean proportionals in the ratio of BC to BM will fall, as are the number of the intermediate terms in either series between these terms and A. Thus between FG and FO fall the mean proportionals FR, FS, and between KL, KQ, the mean proportionals KT, KV, KW, KX. Here the differences GR, RS, SO will be equal in number to k , by which is denoted the distance of the term FG from A, and the differences LT, TV, VW, WX, XQ will be equal in number to b . Now KL is to FG as LT to GR, and LT, TV, VW, WX, XQ are in the same continued proportion as GR, RS, SO: therefore KL is to FG as LW, the sum of LT, TV, VW, whose number is k , to GO, the sum of GR, RS, SO, whose number is likewise k . But the ratio of LQ to GO is compounded of the ratio of LQ to LW, and of that of LW to GO. Now the ratio of LQ to LW is greater than the ratio of b to k ; therefore the ratio of LW to GO being the same with that of KL to FG, the ratio of LQ to GO is greater than that compounded of the ratio of b to k , and of that of KL to FG; that is, greater than the ratio of $b \times KL$ to $k \times FG$.

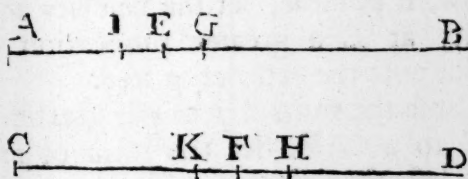
11. Again: KQ is to FO as QX to OS; therefore QX, XW, WV, VT, TL being in the same continued proportion with OS, SR, RG; KQ is to FO as QV, the sum of QX, XW, WV whose number is k , to GO. But the ratio of LQ to GO is compounded of the ratio of LQ to QV, which is less than the ratio of b to k , and of the ratio of QV to GO, or of the ratio of KQ to FO. Therefore the ratio of LQ to GO is less than that of $b \times KQ$ to $k \times FO$.

12. Again, I say, that LQ and GO may be taken so small, that the ratio of LQ to GO shall be less than any ratio, that shall be proposed greater than the ratio of $b \times KL$ to $k \times FG$; or greater than any ratio, that shall be proposed less than the ratio of $b \times KQ$ to $k \times FO$.

13. Let DE, DN be the terms in these series's at the same distance from A, as KL is from FG; then DE will be to A as KL to FG, and $b \times DE$ to $k \times A$ as $b \times KL$ to $k \times FG$; likewise DN to A as KQ to FO, and $b \times DN$ to $k \times A$ as $b \times KQ$ to $k \times FO$.

14. Now in the first place take $b \times DN$ in a less ratio to $k \times A$, than the ratio proposed greater than that of $b \times KL$ to $k \times FG$. Then the ratio of $b \times KQ$ to $k \times FO$ will be less than the ratio proposed; but the ratio of LQ to GO is less than that of $b \times KQ$ to $k \times FO$, and therefore will be less than the ratio proposed.

15. In the next place, let $b \times DE$ be taken to $k \times A$ in a greater ratio than the ratio proposed less than that of $b \times KQ$ to $k \times FO$. Then will the ratio of $b \times KL$ to $k \times FG$ be greater than that now proposed. But the ratio of LQ to GO is greater than that of $b \times KL$ to $k \times FG$, consequently greater than that now proposed.



16. Now in the figure at § 16. of this book, AE being denoted by x , let CF be denoted by

$$\frac{x^{\frac{m}{n}}}{a^{\frac{m-n}{n}}}, \quad m \text{ and } n \text{ representing any two whole numbers.}$$

Then AE and CF will be two terms in a series of proportionals beginning from a , the number n denoting the place of AE, and m the place of CF. Here I say, the velocity, wherewith the point describing CD moves at F, is to the velocity of the point moving on AB at E, as $m \times CF$ to $n \times AE$;

that is, as $m \times \frac{x^{\frac{m}{n}}}{a^{\frac{m-n}{n}}}$ to $n \times x$, or as $\frac{m}{n} x^{\frac{m-n}{n}}$ to $a^{\frac{m-n}{n}}$.

17. In the first place G and H, as likewise I and K being other contemporary positions of the points moving on AB and CD; AG and CH will be terms in a series of proportionals beginning from a , and likewise AI and CK terms in another series of proportionals also beginning from a situated in like manner, as the terms AE and CF in the series, to which they belong. Therefore, if m be greater than n , from what has above been written, FH bears a greater proportion to EG, and KF a less proportion to EI than $m \times CF$ bears to $n \times AE$. Consequently, if IE be equal to EG, KF will be less than FH; insomuch that if the point on AB moves with a uniform velocity, the point on CD moves with a velocity continually accelerated.

18. Now, if possible, let the velocity at F bear to the velocity at E a greater proportion than that assigned, suppose the ratio of p to q .

19. Because the ratio of p to q is greater than that of $m \times CF$ to $n \times AE$, let the ratio of $m \times CH$ to $n \times AE$ be less than the ratio of p to q . Then, by what has been above written, the ratio of FH to EG is less than the ratio of $m \times CH$ to $n \times AE$, consequently less than the ratio of p to q , or of the velocity at F to the velocity at E; which is absurd, the first of these ratios being greater than the last.

20. Again, suppose the velocity at F bear to the velocity

velocity at E a less proportion than that assigned, suppose the ratio of r to s .

21. The ratio of r to s being less than that of $m \times CF$ to $n \times AE$, let the ratio of $m \times CK$ to $n \times AE$ be greater than the ratio of r to s . Then the ratio of KF to IE will be greater than the ratio of $m \times CK$ to $n \times AE$; consequently greater than the ratio of r to s , or of the velocity at F to the velocity at E; which is absurd, the first of these ratios being less than the last.

22. If the point on the line AB should move from I to G, not with a uniform velocity, but with a velocity continually increasing; since, when IE is equal to EG, KF is less than FH, the point on CD will move with a velocity more accelerated; and if the point moving from I to G proceed with a decreasing velocity, the motion from K to H will at least decrease slower: insomuch that in these cases also the proportion of FH to EG will be greater, and that of KF to IE less than that of the velocity at F to the velocity at E. Therefore the demonstration will here proceed in the same manner as before.

23. If m be less than n , KF will be greater than FH, and the point on the line CD move with a decreasing velocity, when the motion from I to G is uniform; but the demonstration will here also proceed after the like manner. Nor will it be different when one of the numbers m or n is negative.

24. The doctrine of fluxions, as delivered by Sir *Isaac Newton*, consists of two parts; the form of conception we have above described, and the method of applying it for the solution of mathematical problems. The surprizing improvements Sir *Isaac Newton* has made in the analytical part of geometry by these principles, his immortal treatise on the quadrature of curves abundantly sets forth. But the author of this discourse designing only to consider the mode of conception proposed in this doctrine, he

has avoided any particular explanation of the forms of calculation, that having been largely performed by others. But as the introduction to this subject, the most read, has accommodated these calculations to the system of indivisibles*, it has occasioned the true mode of conception, so necessary to give this doctrine a place in geometry, to be very much neglected. The author therefore has shewn at large, how we may carry along with us the genuine form of conception in the application of this doctrine to the mensuration of curvilinear spaces, the drawing of tangents, and other problems, to which these principles are to be applied.

25. FLUXIONS not affording the most convenient means for synthetic demonstration, Sir *Isaac Newton*, who in all his writings has shewn the strongest desire of using brevity, invented still another form of reasoning, from what he calls the prime and ultimate ratios of the increments or decrements of varying quantities, whereby to avoid the length of the ancient demonstrations by exhaustions.

26. This method is as essentially different from that of indivisibles, as the former; but however, requires somewhat greater attention to avoid falling into that faulty manner of conception. It depends on the first lemma of his mathematical principles of natural philosophy, the genuine meaning of which is, That those quantities are to be esteemed ultimately equal, and those ratios ultimately the same, which are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference.

* Analyse des infiniment petits. Par le Marquis de l'Hôpital.

27. What Sir *Isaac Newton* intends we should understand by the ultimate equality of magnitudes, and the ultimate identity of ratios proposed in this lemma, will be best known from the demonstration annexed to it. By that it appears, Sir *Isaac Newton* did not mean, that any point of time was assignable, wherein these varying magnitudes would become actually equal, or the ratios really the same; but only that no difference whatever could be named, which they should not pass. The ordinate of any diameter of an hyperbola is always less than the same continued to the asymptote; yet the demonstration of this lemma can be applied, without changing a single word, to prove their ultimate equality. The same is evident from the lemma immediately following, where parallelograms are inscribed, and others circumscribed to a curvilinear space. Here the first lemma is applied to prove, that, by multiplying the number and diminishing the breadth of these parallelograms *in infinitum*, that is, perpetually and without end, the inscribed and circumscribed figures become ultimately equal to the curvilinear space, and to each other; whereas it is evident, that no point of time can be assigned, wherein they are actually equal; to suppose this were to assert, that the variation ascribed to these figures, though endless, could be brought to a period, and be perfectly accomplished; and thus we should return to the unintelligible language of indivisibles. The excellence of this method consists in making the same advantage of this endless approximation towards equality, as by the use of indivisibles, without being involved in the absurdities of that doctrine. In short, the difference between these two may be thus explained.

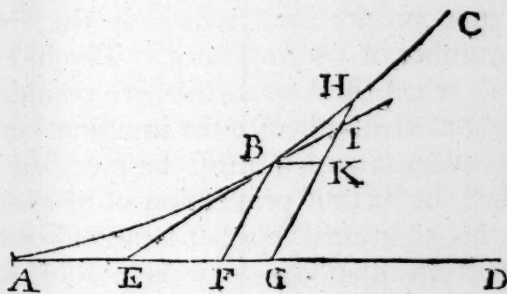
28. There are but three ways in nature of comparing spaces: one is by shewing them to consist of such, as by imposition on each other will appear to occupy the same place: another is by shewing their

proportion to some third; and this method can only be directly applied to the like spaces as the former, for this proportion must be finally determined by shewing, when the multiples of such spaces are equal, and when they differ: the third method, to be used where these other two fail, is by describing upon the spaces in question such figures, as may be compared by the former methods, and thence deducing the relation between those spaces by that indirect manner of proof commonly called *deductio ad absurdum*; and this is as conclusive a demonstration, as any other, it being indubitable, that those things are equal, which have no difference. Thus *Euclide* and *Archimedes* demonstrate all they have writ concerning the comparison and mensuration of curvilinear spaces. The method advanced by *Sir Isaac Newton* for the same purposes differs from theirs, only by applying this indirect form of proof to some general propositions, and from thence deducing the rest by a direct form of reasoning. Whoever compares the fourth of *Sir Isaac Newton's* lemmas with the first, will see, that the proof of the curvilinear spaces there considered having the proportion named depends wholly upon this, that if otherwise the figure inscribed within one of them could not approach by some certain distance to the magnitude of that space: and this is precisely the form of reasoning, whereby *Euclide* proves the proportion between different circles. As this method of reasoning is very diffusely set out in the writings of the ancients, and *Sir Isaac Newton* has here expressed himself with that brevity, that the turn of his argument may possibly escape the unwary; the author has recommended the reading the ancients, as the best introduction to the knowledge of this method. The impossible attempt of comparing curvilinear spaces without having any recourse to the forementioned indirect method of arguing produced the absurdity of indivisibles.

29. As the magnitudes called in this lemma ultimately equal may never absolutely exist under that equality; so the varying magnitudes holding to each other the variable ratios here considered may never exist under that, which is here called their ultimate ratio. Of this Sir *Isaac Newton* gives an instance, which the author of this treatise has repeated after him, from lines increasing together by equal additions, and having from the first a given difference. For the ultimate ratio of these lines in the sense of this lemma, as Sir *Isaac Newton* himself observes, will be the ratio of equality, though these lines can never have this ratio; since no point of time can be assigned, when one does not exceed the other.

30. In like manner the quantities, called by Sir *Isaac Newton* vanishing, may never subsist under that proportion, here esteemed their ultimate.

31. In § 137. of this treatise, where BF bears the same proportion to the subtangent FE, as that where-with the lines HK, KB vanish, these lines must not be conceived, by the name of evanescent or any other appellation, ever to subsist under that proportion;



for should we conceive these lines in any manner to subsist under this proportion, though at the instant of their vanishing, we shall fall into the unintelligible notion of indivisibles, by endeavouring to represent to the imagination some inconceivable kind of existence of these lines between their having a real mag-

magnitude, and becoming absolutely nothing. Sir *Isaac Newton* was himself apprehensive, that this mistake might be made; for as he thought fit (in compliance with the bad taste, which then prevailed) to continue the use of some loose and indistinct expressions resembling those of indivisibles, for which he has himself apologised, he expressly cautions us against misinterpreting him in this manner, when he says: *Si quando dixerō quantitates quā minimas, vel evanescentes, vel ultimas, cave intelligas quantitates magnitudine determinatas, sed cogita semper diminuendas sine limite**. Thus expressly has he declared to us, that vanishing quantities, or whatever other less accurate appellation he names them by, are to be considered as indeterminate quantities bearing to each other under their different magnitudes different proportions; and that the limit of these proportions, which the quantities themselves can never obtain, is that, for the sake of which, these quantities are considered: insomuch, that since these quantities have different proportions, while they obtain the name of vanishing quantities, the author of this treatise has justly observed the term ultimate to be necessarily added to denote that proportion, which is the limit of an endless number of varying ones. The like remark is necessary, when these quantities are considered in the other light as arising before the imagination: for then the proportion intended must be specified by calling it the first or prime proportion of these quantities. And as this additional epithet is necessary to express the proportion intended, so it is absurd to apply it to the quantities themselves; as Sir *Isaac Newton* says, there are *rationes primæ quantitatum nascentium*, but not *quantitates primæ nascentes*†.

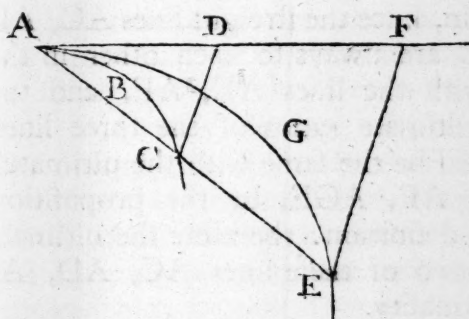
32. The author of this treatise thought, the readiest method to guard against all errors of this kind

* Princip. Phil. Lem. xi. in schol.

† Philosophical Transact. No. 342. pag. 205. or Commer. Epistolic. p. 38.

was to represent the principles of prime and ultimate ratios, and their application to geometrical subjects under such a form of expression, as might be so totally inconsistent with indivisibles, as not to be capable by any misinterpretation of being accommodated to that erroneous manner of conception. But at the same time he took care, that his phrase should not differ essentially from Sir *Isaac Newton's*; as will appear by comparing the two modes of expression in the following instance, being the seventh of Sir *Isaac Newton's* lemmas concerning prime and ultimate ratios, which is to this effect.

33. If any arch ABC be subtended by a chord AC, and at A, where the curvature is understood to be uninterrupted, it be touched by the straight line AD; if the point C be supposed to approach towards A, till those points coincide, the ultimate ratio of the chord, arch, and tangent will be the ratio of equality; provided the tangent AD be terminated



by some line DC drawn from C, the extremity of the arch, so as always to make some angle with the tangent AD.

34. This Sir *Isaac Newton* thus demonstrates.

35. While the point C approaches to the point A, suppose AC and AD always to be produced to the distant points E and F, and EF to be drawn parallel to CD; and let the arch AGE be always similar to the

the arch ABC. Now when the points A and C coalesce, the rectilinear angle under EAF must vanish; therefore the right lines AE, AF, which are always of a finite magnitude, and also the intermediate arch AGE must coincide, and consequently become equal. Therefore the ultimate ratios of the straight lines AC, AD, and the curve ABC, all which vanish, when the point C coincides with A, will be the ratio of equality.

36. Now in the phrase of this book, it must be said, that the arch AGE can never be equal to the chord AE; nor the chord AE equal to the tangent AF, unless when the angles under AEF and under AFE chance to be equal. But, by causing the point C to approach the point A, the ratios of these three lines to each other may at last be brought nearer to the ratio of equality than to any other whatever. Therefore, according to the definition in § 124. of this treatise, the ultimate ratio of any one of these three lines to either of the others is the ratio of equality. And again, since the straight lines AC, AD, and the arch ABC are always to each other in the same proportion with the lines AE, AF, and the arch AGE; the ultimate ratios of the three lines AC, AD, ABC will be the same with the ultimate ratios between AE, AF, AGE, by the proposition subjoined to that definition: therefore the ultimate ratio between any two of these lines AC, AD, ABC is the ratio of equality.

37. By this instance it is manifest, that the style, under which the author has treated this subject, is only an interpretation of Sir *Isaac Newton's*; and such an interpretation he thought alone sufficient to answer the purpose of his writing. No objection had been made against the truth of the conclusions drawn from this method of reasoning. Indeed all error of that kind may be avoided by proper circumspection even in the use of indivisibles. But as the

only suspicion lay against the propriety in the conception and expression advanced in this doctrine; if his interpretation involves in it no perplexed or imperfect ideas, which the author flatters himself will be allowed, it is a full justification of this method.

38. The author believes himself as desirous, as any one can be, to preserve propriety of expression and perspicuity of conception in mathematical subjects. He therefore freely acknowledges, that he has not vindicated this doctrine, unless he shall be found to have accommodated to it a clear and unexceptionable mode of expression, nor freed the inventor from censure, unless he has also shewn, that the turn of phrase recommended by Sir *Isaac Newton*, without any forcible construction, is adapted to convey the same ideas. The author has often lamented the negligence of geometrical writers in regard to their style and diction. The introduction of the terms of arithmetic into geometry by *Des Cartes*, and the favourable reception of the unintelligible jargon of indivisibles have overwhelmed the mathematical sciences with such a profusion of intricate and inconceivable forms of speaking, that they began to be no longer that guide to sound reasoning, which they had hitherto been thought. To restore in some measure geometry from this corruption was the design of Sir *Isaac Newton* in advancing the doctrine of prime and ultimate ratios; and how far the author of this treatise has proved him successful, must be submitted to the judgment of the publick.

39. Sir *Isaac Newton* has made use of prime and ultimate ratios chiefly for synthetic demonstrations; yet as they furnish a direct manner of proof, it is manifest they may be also applied to the analysis of problems.

40. To compleat the design of this treatise, it was necessary to explain Sir *Isaac Newton*'s own demonstrations of his rules for finding fluxions; for which
purpose

purpose nothing more was thought necessary than to dilate Sir *Isaac Newton*'s words by a small paraphrase. For though Sir *Isaac Newton* demonstrates these rules upon the principles of prime and ultimate ratios, yet after what had been written, it seemed scarce possible any longer to confound these two methods together. Indeed when the author considers, how expressly Sir *Isaac Newton* himself has distinguished them, he owns himself surprised, that this mistake should ever have been made.

41. The treatise concludes with an explication of what is to be understood by the momentum of quantities, which is a term appertaining to the doctrine of prime and ultimate ratios only*.

42. The term momentum being of no other use than to give the expression in particular cases greater brevity, the truth of this doctrine has no dependance on the sense of this term; therefore it was not necessary to be taken notice of in this general account; but as it has been conceived to contain something very abstruse, if not unintelligible, the author has explained it at large.

43. And here the author confesses, he met with the greatest difficulty; for it must be acknowledged, that Sir *Isaac Newton*'s description is capable of an interpretation too much resembling the language of indivisibles. But were we to allow Sir *Isaac Newton*'s definition of momenta to be founded entirely upon that erroneous doctrine, the utmost, that will follow from so large a concession, is only this, that though he invented the doctrines of fluxions, and of prime and ultimate ratios; yet he has demonstrated some of the propositions in his mathematical prin-

* These symbols o and $\dot{x}$ are put for things of a different kind: the one is a moment, the other a fluxion or velocity.

Mr. *Leibnitz* hath no symbols of fluxions in his method. He used the symbols of moments or differences dx , dy , dx , *Philos. Transf.* N. 342. p. 204. and *Commer. Epistolic.* p. 37, 38.

ciples of natural philosophy by the means of indivisibles. It cannot hence be inferred, that his doctrine of prime and ultimate ratios has any connexion with indivisibles, or was insufficient for these cases. The author of this treatise has certainly freed the doctrine from this latter imputation by shewing, that such a sense may be put upon the word momentum, as will render those very demonstrations of Sir *Isaac Newton*, where this word is used, as just as any other upon the principles of prime and ultimate ratios. For this purpose the author, without confining himself to the express words of Sir *Isaac Newton*, has given such a definition of this term, as he thought most suitable to the principles of that method.

44. The author of this treatise undertook not to prove, that Sir *Isaac Newton* has never deviated from the utmost propriety of expression, nor that he never demonstrated any proposition upon the principles of indivisibles. He knows Sir *Isaac Newton* did sometimes make use of that method of reasoning; but this he contends for, that the methods under consideration are absolutely different from that doctrine.

45. When Sir *Isaac Newton* first invented his method of fluxions, he demonstrated the rules of that method by indivisibles, as he acknowledges himself*. That in his calculus he made use of the character or symbol o to denote an infinitely small quantity. This he did both in his treatise of series and fluxions written in the year 1671, and also in his treatise on quadratures as he at first writ it, from which we have a

* *Philos. Transf.* No. 342. p. 205. or *Com. Epist.* p. 38. In his (*Newton's*) calculus there is but one infinitely little quantity represented by a symbol, the symbol o .

Mr. *Newton* used the letter o in his analysis, and in his book of quadratures, and in his *principia philosophiæ*, and still uses it in the very same sense as at first. *Philos. Transf.* *Ibid.* p. 204. or *Com. Epistol.* p. 37.

transcript in Dr. *Wallis** not as he afterwards corrected it in his own edition of that treatise. He has likewise made use of the same in one proposition of his *Principles of natural philosophy*†. And he informs us, that though in demonstrating any proposition he chose to use the letter *o* for a finite moment of time, &c. and perform the whole calculation by the geometry of the ancients in finite figures or schemes without any approximation, and when the calculation was at an end, and the equation reduced, to suppose the moment *o* to decrease *in infinitum* and vanish, yet in the investigation, when it would make dispatch, he would suppose the moment *o* to be infinitely little, using all manner of approximation, which he conceived would produce no error in the conclusion‖. Accordingly we find in his treatise of quadratures, he freed his demonstrations from this defect, under which they first laboured; and the proposition of his principles of philosophy, where he continued the use of indivisibles, is only the analysis of a problem.

46. Thus it appears, that Sir *Isaac Newton* did sometimes allow himself the use of indivisibles; but it also appears, that he always had a dislike to that method, as we learn from his own words, when he says, Since we have no ideas of infinitely little quantities, he introduced fluxions, that he might proceed by finite quantities as much as possible§. But as the brevity, wherewith he chose to write, obliged him still to have recourse to indivisibles for demonstrating the rules of this new method, he at length invented his other method of prime and ultimate ratios, and thereby entirely got over that difficulty.

* Oper. Vol. II. p. 392.

† Lib II. prop. 10.

‖ Philos. Transf. No. 342. p. 179. or Com. Epistol. p. 9.

§ Philos. Transf. Ibid. p. 205. or Com. Epistol. p. 38.

47. We may likewise hence learn, how it came to pass, that his definition of momenta should contain expressions bearing some analogy to those of indivisibles: for he informs us, that originally he used the word moment in a sense agreeable to that doctrine; telling us, That from the moments of time he gave the name of moments to the momentaneous increases, or infinitely small parts generated in moments of time*; though in his principles of philosophy he directs us to interpret his meaning according to the doctrine of prime and ultimate ratios, where he says, *neque spectatur magnitudo momentorum sed prima nascentium proportio*†; and the author of the present treatise has accommodated his description of momenta to this, which he conceives to be Sir Isaac Newton's intention.

48. As the proportion between the increments of magnitudes is in this doctrine considered only for discovering, what is here called their ultimate ratio; when the real proportion of these increments is not to be expressed, but by terms too complex, it is convenient, by neglecting some superfluous part of the increments, or by a proper addition to them, to form new quantities, which shall not only bear to each other a more simple proportion, but the ultimate ratio also of each quantity, thus formed, to the increment, whence it is deduced, shall be the ratio of equality: for by the proportion of such quantities the ultimate ratios of the increments are more readily assignable. These are the quantities called momenta. For example; if the increment of any line denoted by x be represented by o , the increment of the line denoted by any power x^n will be $nx^{n-1}o + n \times \frac{n-1}{2} x^{n-2} oo + \mathcal{E}c.$ Here as the ultimate ratio of the

* Phil. Transf. Ibid. p. 178. or Com. Epist. p. 7.

† Lib. II. Lem. 2.

first of these increments to the last is that of o to $nx^{n-1}o$, the line denoted by this term $nx^{n-1}o$ only is sufficient to express that ultimate proportion, and therefore may be assumed for the momentum of x^n ; and then, to preserve a similitude of phrase, the entire increment of x is also to be called the momentum of that line.

49. Although it is a great mistake to suppose the validity either of the doctrine of fluxions, or of that of prime and ultimate ratios, to depend upon what Sir *Isaac Newton* has demonstrated concerning the momenta of quantities; yet since his demonstration of the momentum of a rectangle had been controverted, the author has given a brief account of the principles, upon which that demonstration proceeds. And this may be represented more at large as follows.

50. To give this demonstration its utmost extent, suppose some third variable line Z , to which A and B , the sides of the rectangle in question, are in any manner related; and let a and b not be the real increments of A and B , but bear to the increment of Z the most simple relation, whereby they can express the ultimate ratio of the increments of A and B to the correspondent increment of Z ; then may a and b be called the momenta of A and B respectively. And since, at the same magnitudes of Z , A , and B the ultimate ratios between their decrements are the same with those between their increments, by the same magnitudes a and b may be also expressed the ultimate ratios of the decrements of A and B to the correspondent decrement of Z .

51. In like manner the ultimate ratio of the increment of the rectangle under A , B to the correspondent increment of any other rectangle under A and some given line M will be the same with the ultimate ratio of the decrement of the rectangle $A \times B$ to

to the correspondent decrement of the rectangle $A \times M$ at the same magnitudes of A and B . Therefore the ultimate ratio of the increment of $A \times B$ to the correspondent increment of $A \times M$, will be the same with the sum of such increment and decrement of $A \times B$ to the sum of the correspondent increment and decrement of $A \times M$.

52. Farther, the ultimate ratio of the increment of $A \times B$ to the correspondent increment of $A \times M$ will be the same with the ultimate ratio of that augmentation, which the rectangle under A, B will receive by increasing the sides, either by their respective momenta, or by analogous parts of those momenta, to the augmentation, which the rectangle under A, M will receive from the moment of A , or a similar part thereof. Therefore the ultimate ratio of the increment of $A \times B$ to the correspondent increment of $A \times M$ will be the same with the ultimate ratio of that augmentation, which the rectangle $A \times B$ will receive from increasing its sides A and B by half their momenta a and b , to the augmentation, which $A \times M$ will receive from increasing A by half its momentum a . In like manner the ultimate ratio of the decrement of $A \times B$ to the correspondent decrement of $A \times M$ will be the same with the ultimate ratio of that diminution, which the rectangle $A \times B$ will receive by taking from each of its sides half its momentum, to the diminution, which the rectangle $A \times M$ will receive from half the momentum of A .

53. Hence it follows, that the ultimate ratio of the increment of $A \times B$ to the correspondent increment of $A \times M$ is the same with the ultimate ratio of the sum of the augmentation and diminution, which the rectangle $A \times B$ will receive from half the momenta of its sides, to the sum of the augmentation and diminution, which the rectangle $A \times M$ will receive from half the mo-

mentum of A ; that is, the same with the ultimate ratio of $\frac{1}{2}a \times B + \frac{1}{2}b \times A + \frac{1}{4}ab$ and $\frac{1}{2}a \times B + \frac{1}{2}b \times A - \frac{1}{4}ab$ together, or of $a \times B + b \times A$, to $a \times M$.

54. As the square is comprehended under this general proposition for all rectangles, so by a similar artifice we may demonstrate the momentum of any other power. For instance, in the cube of any variable quantity A , whose increment or moment is o , if we divide that increment into two parts p and q , that the ratio of p to q may be subduplicate of the ratio of $3A - q$ to $3A + p$; $3A \times pp + p^3$ will be equal to $3A \times qq - q^3$, whereby the cube of $A + p$, or $A^3 + 3A^2p + 3Ap^2 + p^3$, will exceed the cube of $A - q$, or $A^3 - 3A^2q + 3Aq^2 - q^3$, by $3A^2 \times p + q$, or $3A^2 \times o$, the momentum of A^3 .

55. Here dividing the increment o into two equal parts will not answer the purpose intended; for by deducting the cube of $A - \frac{1}{2}o$, or $A^3 - \frac{3}{2}A^2o + \frac{3}{4}Ao^2 - \frac{1}{8}o^3$, from the cube of $A + \frac{1}{2}o$, or $A^3 + \frac{3}{2}A^2o + \frac{3}{4}Ao^2 + \frac{1}{8}o^3$, the residue will be $3A^2o + \frac{1}{4}o^3$, exhibiting more than is necessary for the momentum of the cube of A ; for the momentum should be the simplest term, whereby the intended ultimate ratio can be expressed.

56. In other compound quantities the demonstration may be conducted upon the same model by such a division, as each particular case shall require, of the moments of the original quantities, whereof those under consideration are compounded; but when such division is of too perplex a kind, another method of demonstrating is to be preferred.

57. This is abundantly sufficient for explaining the demonstration in question. And as the author of this discourse presumes, he has given throughout a genuine representation of Sir *Isaac Newton's*

real and sole intention: so he hopes it will appear, that however less exact in the choice of his expressions that great man may have been at any other time; yet when he purposely describes these methods, and explains their principles, he is not only perfectly consistent with himself, but has also delivered his meaning with such perspicuity, that we need not have recourse to any deference for his authority to be fully satisfied of the truth of these doctrines.



A

R E V I E W

O F

Some of the principal Objections that have been made to the Doctrines of Fluxions and Ultimate Proportions; with some Remarks on the different Methods, that have been taken to obviate them.

First published in The Present State of the Republick of Letters for December 1735.

1. **T**HE objections, that have been made to the conception and nature of fluxions, have principally arisen, either from confounding this doctrine with the method of indivisibles, and the differential calculus of foreigners, or from supposing (as fluxions are said to be velocities) that the fluxion of a quantity, and the velocity of a quantity, were synonymous terms; forgetting that it is not to the quantities themselves, but to their degree of increase or decrease, that this velocity intended by the fluxion is ascribed. But as these mistakes can be no longer made without the greatest negligence or dissingenuity; it may be reasonably supposed, that no exception of this kind will for the future be insisted on. We shall there-

therefore at this time confine ourselves to the objections of another kind; such as have been urged against those operations, by which the proportion of the fluxions of different flowing quantities are determined.

2. These objections have been particularly levelled at that expression of Sir *Isaac Newton*, *Fluxiones sunt in prima ratione augmentorum nascentium*, — or in *ultima ratione partium evanescentium**. Which being usually thus translated, that *fluxions are in the prime ratio of the nascent augments*, — or in the *ultimate ratio of the evanescent parts*†; it has been from hence asked, What these nascent or evanescent parts, augments or decrements are? If of any magnitude, then it will be confessed by the espousers of this doctrine, that their ratio is not the same with the ratio of the fluxions. If it is answered, that they are of no magnitude; it is then said, that to talk of the ratio of nothings, is such a strain of language, as it is supposed, the warmest followers of the inventor will scarce undertake to defend.

3. To obviate this objection, two explanations have been given of this quotation.

4. The first endeavours to shew, how this imagined difficulty may be avoided, not by considering these nascent augments and evanescent decrements as being actually vanished, in which case they can have no proportion, nor yet as being of any real magnitude, when their proportion cannot be the same with the proportion of the fluxions; but by supposing that there can be represented to the mind some intermediate state of these augments or decrements at the very instant, in which they vanish.

5. Another writer, in his discourse concerning the nature and certainty of Sir *Isaac Newton's* me-

* *Introduct. ad Quadrat. Curv.*

† *Harris's Lexicon Technicum*, Vol. II. in the word *Quadrature*.

thods of fluxions and of prime and ultimate ratios, has endeavoured to shew, that this objection is founded on an erroneous hypothesis; for that by the ultimate proportion of varying quantities was only meant the limit of their varying proportions, and not a proportion, that these varying quantities could ever exist under during their variation; and consequently that the true explication of this passage should be, fluxions are in that proportion, which is the ultimate to all those varying proportions, that the decrements bear to each other, whilst they are vanishing or diminishing; that is, the limit of the proportions, that the decrements bear to each other, as they diminish, is the true proportion of the fluxions. By this interpretation, which is supported by Sir *Isaac Newton's* own words, the above-mentioned objection immediately falls to the ground; since it is altogether founded on the supposition, that the decrements in their imagined evanescent state did really bear to each other the proportion of the fluxions; whereas this passage, when truly understood, does not suppose, that the decrements can, in any circumstance whatever, bear to each other that proportion; but asserts on the contrary, that the proportion of the fluxions is only a proportion limiting all the varying proportions, that these decrements have to each other in their various degrees of diminution.

6. At the same time that this objection was raised against the doctrine of fluxions, the method of prime and ultimate ratios was excepted to: in particular it was urged, that the quantities or ratios, asserted in this method to be ultimately equal, were frequently such, as could never absolutely coincide. As for instance, the parallelograms inscribed within the curve in the second *Lemma* of the first book of Sir *Isaac Newton's Principia*, cannot by any division be made equal to the curvilinear space they inscribed; whereas in that *Lemma* it is asserted, that they are ultimately equal to that space.

7. And

7. And here again two different methods of explanation have been given. The first, supposing that by ultimate equality a real assignable coincidence is intended, asserts, that these parallelograms and the curvilinear space do become actually, perfectly, and absolutely equal to each other. But the author of the above-mentioned treatise has given such an interpretation of this method, as did no ways require any such coincidence.

8. In his explication of this doctrine of prime and ultimate ratios he defines the ultimate magnitude of any varying quantity to be the limit of that varying quantity, which it can approach within any degree of nearness, and yet can never pass. And in like manner the ultimate ratio of any varying ratio is the limit of that varying ratio. These definitions being premised, he demonstrates, that when varying magnitudes keep always in the same proportion, then their ultimate magnitudes will be in that same proportion; and that all the ultimate ratios of any particular varying ratio is the same. From these propositions thus established, all, that has at any time been demonstrated by the ancient method of exhaustions, may be most easily and elegantly deduced; and that by a method not yielding in brevity to the artless inconclusive process by indivisibles.

9. It is evident, that no coincidence of the varying quantity and its limit is at all supposed necessary in this method; since the ultimate magnitude of a varying one is not to be denominated from any such coincidence of the varying one with it, but from its being that magnitude, which the varying one can approach within any degree of nearness.

10. It has been supposed, that the accuracy of the demonstrations founded on this doctrine did in reality depend on this coincidence; but this mistake has arisen from forgetting, that the demonstrations deduced from this method are applied to the limits of
varying

varying magnitudes and proportions, and not to the varying quantities or proportions themselves.

11. Thus, if by means of a polygon described about a circle, we were to demonstrate the equality of that circle to a triangle, having for its base the circumference of that circle, and the semidiameter for its altitude, the proof would not be founded on the real coincidence of the polygon and circle, since this could not be effected by any diminution of the sides of the polygon; but its demonstration would altogether proceed by shewing, that as the circumscribed polygon could approach both the circle and triangle within any degree of nearness, and yet could pass neither of them; therefore the circle and triangle, thus shewn to be the limits or ultimate magnitudes of the same varying magnitude, cannot differ from each other.

12. In like manner in demonstrating the proportion that the fluxions of two flowing quantities bear to each other, the demonstration is not founded on the coincidence of the proportion of the decrements with that proportion, which is given for the proportion of the fluxions; for the coincidence of these proportions cannot by any diminution of the decrements be ever effected: but the proof depends upon this, that, since by diminishing the decrements the proportion of those decrements can be brought within any degree of nearness to that given proportion, and also to the proportion of the fluxions, and yet can never pass either of them; therefore that given proportion, and the proportion of the fluxions, cannot differ from each other, they being thus shewn to be each of them the limit or ultimate proportion of the same varying proportion.

13. From hence it appears, that the coincidence of the variable quantity and its limit, could it be always proved, would yet bring no addition to the accuracy of these demonstrations; and since by the
division

division of magnitudes no such coincidence can ever take place, why to the natural difficulty of these subjects should the obscurity of so strained a conception be added? certainly neither brevity, perspicuity, nor exactness can be at all promoted, by supposing in these demonstrations that circumstance to be ever necessary, which in numberless instances is not possible; and which by its taking place or not, can no ways affect the justness of the conclusion.

14. But it has been urged against this explication, that Sir *Isaac Newton* does in his first *Lemma* of his first book assert such a coincidence; and therefore, though the method of managing prime and ultimate proportions here described may be conclusive, yet it is not a true interpretation of Sir *Isaac Newton*.

15. What foundation, there is for this charge, will best appear by considering the *Lemma*; and that this may be done with more convenience, we will insert a literal translation of it.

16. *Quantities, and the ratios of quantities, that during any finite time constantly approach each other, and before the end of that time approach nearer than any given difference, are ultimately equal.*

17. In order that the coincidence between the variable quantity and its limit should be intended in this *Lemma*, it is necessary, that the phrase of *given difference* should mean a difference, that may be taken at pleasure, after the celerity or degree of approach of these quantities or ratios is in every part determined.

18. But if, according to the most usual and authentic signification of this phrase, there is meant by the *given difference*, in this *Lemma*, a difference first assigned, according to which the degree of approach of these quantities may be afterwards regulated; then variable quantities or ratios, and their limits, though they do never actually coincide, will come within the description of this *Lemma*; since the difference

ence being once assigned, the approach of these quantities may be so accelerated, that in less than any given time the variable quantity, and its limit, shall differ by less than the assigned difference.

19. Now that the latter sense is the true interpretation, will appear from the demonstration and application of the *Lemma*.

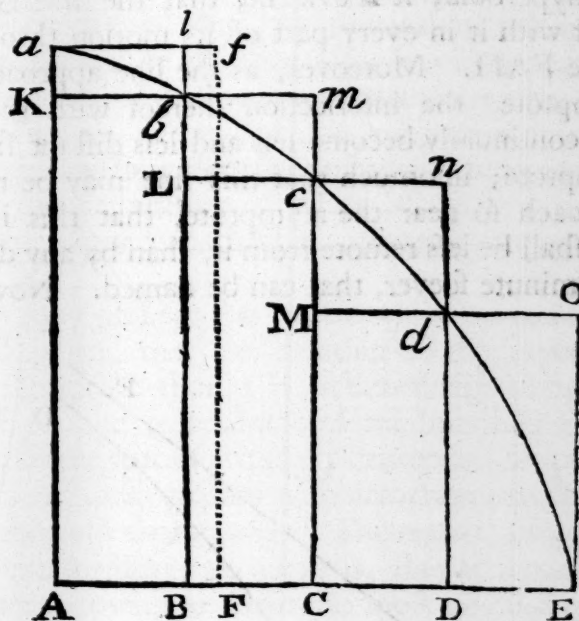
20. In the first place, the demonstration of this Lemma may, without the change of a single word, be applied to prove, that the ultimate ratio of the ordinate of the hyperbola to the same ordinate continued to the asymptote is the ratio of equality; and yet it is confessed, that in this case there can never be an actual coincidence.

21. In the next place, the quantities in many of the succeeding *Lemmas*, to which the first is applied, are such, where the approach is determined by a subdivision into parts; but by this method of proceeding it is obvious, that no coincidence can ever be obtained.

22. However it is said, that by motion this coincidence may be actually made to take place even in these quantities; as, suppose in the second Lemma a point E to describe the line EA with a continued motion in the space of an hour, and let it be conceived, that in every point of time during that hour, a rectangle, as AB/, is raised upon AB, that point of the line EA, which at that point of time is yet undescribed; also upon every other part of the line equal to AB let other rectangles be erected, as in the figure, at the same point of time. It is said that by this means, at the end of the hour, when the point E arrives at A, the curvilinear space and the inscribed figure will actually coincide.

23. To this it may be replied, that supposing the coincidence could by this means take place, it would prove, that no such coincidence was ever intended by Sir Isaac Newton; since had he regarded it as a
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necessary circumstance, he would certainly have applied to this *Lemma* a method of inscribing the figure,

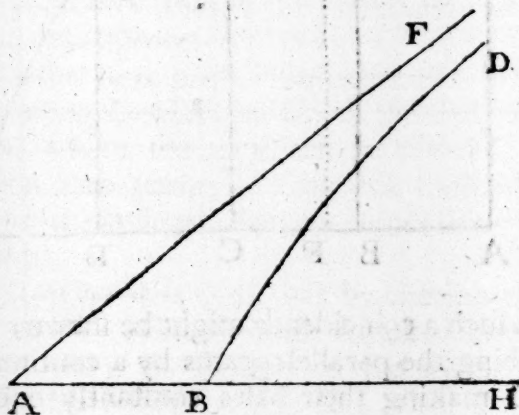


by which such a coincidence might be shewn; whereas by describing the parallelograms by a continual division, and making their bases constantly equal, and always some *aliquot* part of the whole, he has necessarily excluded the description of them by motion, by which means only it is supposed, that this coincidence can be brought about.

24. But farther, this supposed demonstration, that an actual coincidence of the inscribed figure may be effected by the forementioned motion, is really inconclusive; since from a like method of proceeding may be deduced this absurd conclusion, that hyperbolas coincide with their asymptotes.

25. Suppose an hyperbola BD, its diameter AH, and its asymptote AF. Now let the line AH revolve about the center A with an angular motion, till it coincides with the line AF; then, since it is demonstrated

frated by geometers, that every right line drawn within the angle FAH will, if produced, meet with the hyperbola; it is evident, that the line AH will meet with it in every part of its motion through the angle FAH. Moreover, as the line approaches the asymptote, the intersection thereof with the curve will continually become less and less distant from the asymptote; insomuch that this line may be made to approach so near the asymptote, that this intersection shall be less remote from it, than by any distance, how minute soever, that can be named. Now let us



suppose the line AH to employ any given space of time, as an hour, in passing over the angle HAF; then does the intersection of the revolving line with the hyperbola continually approach to the asymptote during the space of this hour, and before the end of the hour this point in the hyperbola will approach nearer to the asymptote than by any difference, that can be proposed; consequently by the method of reasoning above made use of, we must conclude, that at the end of the hour the hyperbola actually coincides with the asymptote.

26. If it be examined, wherein lies the fallacy of these conclusions, it will be found, that though the

OF OBJECTIONS. III

the meeting of the hyperbola and its asymptote, and the coincidence of the inscribed figure and the curvilinear space, seem to be pointed out and determined by this form of reasoning; yet to continue the hyperbola and asymptote till they actually meet, requires the delineation of a line longer than any line, that can be assigned; and to describe a figure within the curve under the supposed circumstance of coincidence, requires the delineation of a line less than any line, that can be assigned: both which operations are equally impossible.

27. It may perhaps be worth while to examine, how it happens, that the meeting of the hyperbola and its asymptote should be acknowledged impossible, and yet the coincidence of the inscribed figure and curvilinear space so strongly contended for, when they each of them require a construction equally inconceivable and unattainable. The reason, I suppose, for this extraordinary partiality is, that as a quantity in augmenting without limit did most obviously pass beyond the utmost stretch of imagination, it was without difficulty granted, that the delineation or conception of any such magnitude was impossible: Whereas, when a quantity diminished without limit, the imagination could trace it during the whole time of its diminution; and consequently the conception of a quantity less than any whatever, has been thought possible by some, who allow the absurdity of pretending to conceive a quantity greater than any whatever.

28. If it be said, that though the hyperbola and its asymptote cannot be described under the circumstance of meeting each other; yet the inscribed figure and the curvilinear space can be described under the circumstance of coincidence; so that the curvilinear space itself is the last form of its inscribed figure.

29. I answer, this is not true; for the supposed last form of the inscribed figure must essentially differ from

from the curvilinear space, the perimeter of the inscribed figure contiguous to the curve being in every description, and consequently in this imagined last equal to the sum of the lines aA , AE : whereas if the curvilinear space was really the last form of the inscribed figure, their perimeters could not differ. Since then the curvilinear space is not the last form of the inscribed figure, and since the last form of this figure cannot be described, but by the delineation or conception of a line less than any line, that can be assigned; it is evident, that the coincidence in this case does equally, with the meeting of the hyperbola and its asymptote, involve an impossibility.

30. But the strongest proof, that Sir *Isaac Newton* does not always consider this coincidence of the variable quantity, or ratio and its ultimate, as necessary in his method, is, that he himself tells us, that if two lines increasing without limit have always a given difference, then their ultimate ratio will be the ratio of equality. Now the phrase of ultimate ratio is peculiar to him and to his method, and cannot possibly be supposed in this place to have a signification different from what, it had in the first and subsequent *Lemmas*; consequently the ultimate ratio is, by his own express description, a ratio, that the variable one, it is ascribed to, cannot always coincide with.

A
DISSERTATION

SHEWING,

That the account of the doctrines of Fluxions, and of prime and ultimate Ratios, delivered in a treatise, entitled, A discourse concerning the nature and certainty of Sir Isaac Newton's methods of fluxions, and of prime and ultimate ratios, is agreeable to the real sense and meaning of their great inventor.

First published in The Present State of the Republick of Letters for April 1736.

I. **T**HE principal pursuit of the geometers of the last century was in search after general methods for discovering the nature and properties of curved figures. Sir Isaac Newton succeeded so happily in this attempt, as to establish a very extensive method of computation for these purposes. And being dissatisfied with the doctrine of indivisibles, or of infinitely small quantities, which had been hither-

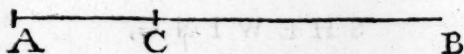
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to employed in these enquiries, he introduced his doctrines of fluxions, and of prime and ultimate ratios, as juster principles, whereon to found this method of computation. The book, we are now going to consider, is confined to the explanation of these principles.

2. The substance of what we have there said with relation to the conception and nature of fluxions, is as follows.



3. Let the line AB be supposed to be tracing out by the motion of a point setting forward from A; then the velocity of that point in any part of its motion, as at C, will be the fluxion of the line AC at that time described.

4. And as the velocity of this point in different parts of its motion may be supposed to be any how increased or diminished, the degree of this increase or diminution at C is the second fluxion of the line AC.

5. Again, since this increase or diminution may be of different degrees at different places, it may itself also have a change, which at different places will be greater or less. And the degree of this change at the place C is the third fluxion of the line AC.

6. The fluxion of other quantities is not the velocity of the motion, whereby they are increased, but the rate of that increase. And here the terms velocity, celerity, and degree of swiftness, which originally belong to actual motion, being applied to this rate of increase in a sense somewhat figurative, Sir *Isaac Newton* in lines chose to call the actual motion, wherewith they are described, the fluxion of those lines, as being an idea more obvious than the rate of their increase, which otherwise might have been assigned

signed for the fluxion of lines, as well as of all other quantities*.

7. The case of lines being thus the simplest in the doctrine of fluxions, we have shewn in our discourse, how that of all other flowing quantities may be reduced to this by causing a point so to pass over any streight line, that its length measured out, while the other flowing quantity is describing, shall augment in the same proportion with such flowing quantity. So that the fluxion or velocity of increase of this fluent, will be ever proportional to the actual velocity of the point describing the line. And from this consideration we demonstrated in particular, that when a curvilinear space is supposed to be described by the uniform and parallel motion of an ordinate, the fluxion of that space will be every where as the length of the ordinate.

8. That our account of fluxions is the same with that given by Sir *Isaac Newton*, will appear from his own words. In the introduction to his treatise of Quadratures he says, *Quantitates mathematicas, non ut ex partibus quam minimis constantes, sed ut motu continuo descriptas hic considero. Lineæ describuntur, ac describendo generantur, non per appositionem partium, sed per motum continuum punctorum, superficies per motum continuum linearum, &c. Considerando igitur, quod quantitates æqualibus temporibus crescentes, & crescendo genitæ, pro velocitate majori vel minori quæ crescunt ac generantur, evadunt majores vel minores; methodum quærebam determinandi quantitates ex velocitatibus motuum vel incrementorum, quibus generantur;*

* As is done in Dr. *Wallis's Works*, Vol. II. p. 391. where it is said of him, *Per fluentes quantitates intelligit indeterminatas, id est quæ in generatione curvarum per motum localem perpetuo augentur vel diminuuntur, et per earum fluxionem intelligit celeritatem incrementi vel decrementi.* And also in the quotation §. 8. from the Quadratures, Sir *Isaac Newton*, speaking of the fluxions of fluents in general, says, *Et earum fluxiones seu celeritates crescendi.*

Et has motuum vel incrementorum velocitates nominando fluxiones, Et quantitates genitas nominando fluentes, &c. Again, in the book itself he says, *Quantitates indeterminatas, ut motu perpetuo crescentes vel decrescentes, id est, ut fluentes vel defluentes, in sequentibus considero, designoque literis z, y, x, v, Et earum fluxiones, seu celeritates crescendi noto iisdem literis punctatis. Sunt Et harum fluxionum fluxiones, sive mutationes magis aut minus celeres, quas ipsarum z, y, x, v fluxiones secundas nominare licet, &c.* Moreover, in some observations he made on a letter of Leibnitz, which were printed at the end of Raphson's *Historia Fluxionum**, he declares, That in a paper written by him so long ago as 1665, the direct method of fluxions was set down in these words: *An equation being given expressing the relation of two or more lines x, y, z, &c. described in the same time by two or more moving bodies, A, B, C, &c. to find the relation of their velocities, p, q, r, &c.* And in the *Philosophical Transactions*, N^o 342, p. 190†, he says, *When he considers lines as fluents described by points, whose velocities increase or decrease, the velocities are the first fluxions, and their increase the second.* And these last have particular regard to the first part of his description of fluxions, where he calls them *velocitates motuum*.

9. Here it most manifestly appears, that our description of fluxions is the very same, Sir Isaac Newton has himself delivered; and as he has never attempted to represent them in any other light, we cannot sufficiently admire, how he came to be charged by a late writer with having given so various and inconsistent accounts of them||. And this notion of fluxions and their different orders is evidently free not only from any impossible, but even obscure sup-

* P. 116, or Recueil de diverses Pieces sur la Philosophie, &c. à Amsterd. 1720, Tom. II. p. 89.

† Or *Commercium Epistolicum* p. 10.

|| *Defense of Free-Thinking in Mathematicks*, p. 41.

positions.

positions. Infomuch that this writer, for the support of his objections against this doctrine, found it necessary to represent the idea of fluxions as inseparably connected with the doctrine of prime and ultimate ratios, intermixing this plain and simple description of fluxions with the terms used in that other doctrine, to which the idea of fluxions has no relation: and at the same time by confounding this latter doctrine with the method of *Leibnitz* and the foreigners, has proved himself totally unskilled in both.

10. These two methods of *Sir Isaac Newton* are so absolutely distinct, that their author had formed his idea of fluxions before his other method was invented, and that method is no otherwise made use of in the doctrine of fluxions, than for demonstrating the proportion between different fluxions. For, in *Sir Isaac Newton's* words*, as the fluxions of quantities are nearly proportional to the contemporaneous increments generated in very small portions of time, so they are exactly in the first ratio of the *augmenta nascentia* of their fluents. With regard to this passage the writer of the Analyst has made a twofold mistake. First, he charges *Sir Isaac Newton*, as saying these fluxions are very nearly as the increments of the flowing quantity generated in the least equal particles of time. Again, he always represents these *augmenta nascentia*, not as finite indeterminate quantities, the nearest limit of whose continually varying proportions are here called their first ratio, but as quantities just starting out from non-existence, and yet not arrived at any magnitude, like the infinitesimals of the differential calculus. But this is contrary to the express words of *Sir Isaac Newton*, who after he had shewn how to assign by his method of

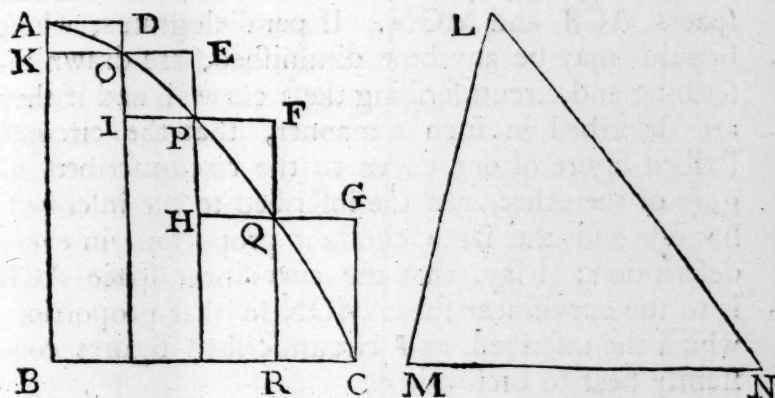
* Fluxiones sunt quam proxime ut fluentium augmenta æqualibus temporis particulis quam minimis genita, &, ut accurate loquar, sunt in prima ratione augmentorum nascentium. *Newton. Introd. ad Quad. Curv.*

prime and ultimate ratios the proportion, that different fluxions have to one another, he thus concludes. *In finitis autem quantitativibus Analysin sic instituere, & finitarum nascentium vel evanescentium rationes primas vel ultimas investigare consuevit geometria veterum: & volui ostendere, quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere.*

11. And this leads us to consider Sir Isaac Newton's doctrine of prime and ultimate ratios, which is of the greatest use in demonstrations relating to curves. It is no other than an abbreviation and improvement of the form of demonstrating used by the ancients on the like occasions. For this reason we premised to our explanation of prime and ultimate ratios a short description of that method, which we shall now consider more at large.

12. As no two different curves can be so laid on each other as to coincide either in whole or in part, it is evident, that the spaces bounded by such curve lines cannot be immediately compared either with each other, or with right-lined figures; nor for the same reason can such spaces be the sums or differences of others, that are capable of being thus compared. In examining then the dimensions and proportions of these curvilinear spaces, some other method must be made use of, than those that are required in the comparison of right-lined figures.

13. Suppose a space bounded by the curve AC, and the right lines AB, BC. Let the base BC be divided into any number of parts, and on those divisions let parallelograms be drawn forming the figures ADOEPFQGCB and KOIPHQRB, the first circumscribing, and the last inscribing the given space ACB. It is now obvious, that by diminishing the breadth of these parallelograms, these figures may be made to differ from the space ACB, and from each other by less than any space, how minute soever, that shall be named; that is, the circumscribed



scribed figure can be made less than any space greater than the curvilinear space, and the inscribed figure greater than any space less than that curvilinear space.

14. If by considering the properties of these inscribed and circumscribed figures, which arise from the nature of the curve, they are adapted to, a right-lined space LMN can be assigned, that shall be greater than every inscribed figure, and less than every circumscribed figure, this right-lined space LMN may be proved to be equal to the curvilinear space ACB.

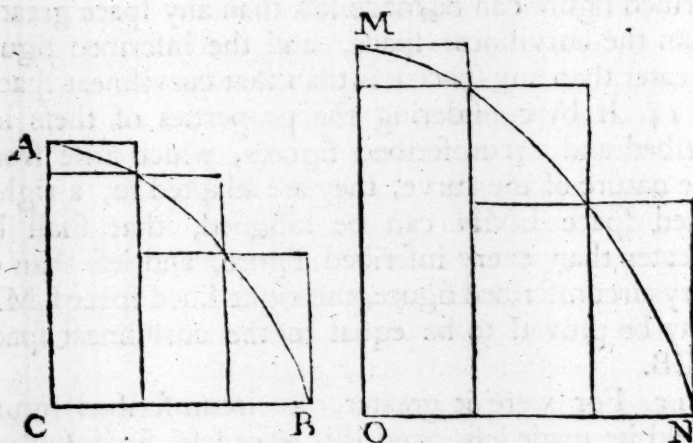
15. For were it greater, a circumscribed figure could be made less; and if it were less, an inscribed figure could be made greater.

16. Instead of both inscribed and circumscribed figures, we might have made use of one of them only, suppose of the inscribed, by proving the space MLN to be greater than every inscribed figure, and also capable of being approached by such a figure within less than any given difference. For thus the space MLN can neither be less nor greater than the curvilinear space. If it were less, an inscribed figure would exceed it; and if it were greater, no inscribed figure could approach it so near as its excess above the curve.

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17. Again,

17. Again, suppose there are two curvilinear spaces, ACB , and MON . If parallelograms, whose breadth may be any how diminished, are drawn inscribing and circumscribing these curves; and if they are described in such a manner, that the circumscribed figure of one curve to the circumscribed figure of the other, and the inscribed to the inscribed, has one and the same constant proportion in every description: I say, that the curvilinear space ACB is to the curvilinear space MON in that proportion, which the inscribed and circumscribed figures constantly bear to each other.



18. For no space greater than ACB can have to MON this proportion; since if it could, a figure might be circumscribed about ACB less than this supposed greater space, and this circumscribed figure to its correspondent figure circumscribing MON would be in the same proportion, as the supposed greater space to the curvilinear space MON ; that is, four quantities being in the same proportion, the first would be less than the third, and the second greater than the fourth. Nor can any space less than ACB have to MON the constant proportion of the figures in

in one curve to the figures in the other. For if it could, a figure might be inscribed within ACB, which would be greater than this supposed lesser space; and this inscribed figure to its correspondent figure inscribing MON would be in the same proportion, as this imagined lesser space to the curvilinear space MON; that is, four quantities being in the same proportion, the first would be greater than the third, and the second less than the fourth. Thus no space but ACB can be to MON in the constant proportion of the circumscribed and inscribed figures.

19. If the proportion of the circumscribed figures, or of the inscribed is not the same in every description, but constantly changing (suppose one of the ratios perpetually increasing, and the other diminishing) as the breadths of the parallelograms are contracted; then it will be shewn by a similar process, that the ratio, which is greater than every increasing proportion, and less than every diminishing proportion, will be the true ratio of the curvilinear spaces.

20. The demonstration may here too proceed by the inscribed or circumscribed figures only, as in the first example, if they bear a constant unchangeable ratio in every description, or if the ratio can be found, which the ratio of those figures does perpetually approach, and to which by diminishing the parallelograms it will at last come nearer, than to any other ratio, that shall be given.

21. Though we have here made use of parallelograms, yet any other method of describing the circumscribed and inscribed figures, may equally take place; provided the figures arising from such description can be made to differ from the curvilinear space by less than any difference whatever, that shall be proposed. And in the description of these figures lies the great artifice of these demonstrations; for they ought to be so drawn, that the right lined space in our first instance,

stance, greater than the inscribed, and less than the circumscribed figure, may from the consideration of these figures be most easily determined; or that the proportions of these figures in the following instances, may in every description be easily assignable.

22. In the manner here described did the ancient geometers demonstrate, whatever they discovered relating to the dimensions or proportions of curve lines, curvilinear spaces, and solids bounded by curved surfaces. And this is the form of demonstration, which is now called the method of exhaustion. But as these demonstrations, by determining distinctly all the several magnitudes and proportions of these inscribed and circumscribed figures, did frequently extend to very great lengths, other methods of demonstrating had been contrived, whereby to avoid these circumstantial deductions. The first attempt of this kind, known to us, is that we mentioned to have been made by *Lucas Valerius*. But afterwards *Cavalierius*, an *Italian*, about the year 1635, advanced his method of indivisibles, in which he proposes not only to abbreviate the ancient demonstrations, but to remove the indirect form of reasoning used by them of proving the equality or proportion between lines and spaces from the impossibility of their having any different relation; and to apply to these curved magnitudes the same direct kind of proof, that was before applied to right lined quantities.

23. This method of comparing magnitudes invented by *Cavalierius*, supposes lines to be compounded of points, surfaces of lines, and solids of planes; or, to make use of his own description, surfaces are considered as cloth consisting of parallel threads, and solids are considered as formed of parallel planes, as a book is composed of its leaves, with this restriction, that the threads or lines, of which surfaces are compounded, are not to be of any conceivable breadth, nor the leaves or planes of solids of any

any thickness. He then forms these propositions, that surfaces are to each other as all the lines in one to all the lines in the other, and solids in like manner in the proportion of all their planes.

24. But this method of indivisibles, as here explained, is manifestly founded on inconsistent and impossible suppositions. For while the lines, of which surfaces are supposed to be made up, are real lines of no breadth; it is obvious, that no number of them can form the least imaginable surface: if they are supposed to be of some sensible breadth, in order to be capable of filling up spaces, that is, in reality to be parallelograms, how minute soever be their altitude, the surfaces may not be to each other in the proportion of all such lines in one to all the like lines in the other; for surfaces are not always in the same proportion to each other with the parallelograms inscribing them.

25. The same contradictory suppositions do obviously attend the composition of solids by parallel planes, or of lines by such imaginary points.

26. This heterogeneous composition of quantity, and confusion of its species, so different from that distinctness, for which the mathematicks were ever famous, was opposed at its first appearance by several eminent geometers, particularly by *Guldinus* and *Tacquet*, who not only excepted to the first principles of this method, but tax the conclusions formed upon it, as erroneous. But as *Cavalierius* took care, that the threads or lines, of which the surfaces to be compared together were formed, should have the same breadth in each (as he himself expresses it) the conclusions deduced by his method might generally be verified by sounder geometry; since the comparison of these lines was in effect the comparing together the inscribed figures.

27. As in the application of this method, error by proper caution might be avoided, the assistance
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it seemed to promise in the analytical part of geometry, made it eagerly followed by those, who were more desirous to discover new propositions, than solicitous about the elegance or propriety of their demonstrations. Yet so strange did the contradictory conception appear of composing surfaces out of lines, and solids out of planes, that in a short time it was new modelled into that form, which it still retains, and which now universally prevails amongst the foreign mathematicians.

28. In this reformed notion of indivisibles, surfaces are now supposed as composed not of lines, but of parallelograms, having infinitely little breadths and solids in like manner as formed of prisms having infinitely little altitudes. By this alteration it was imagined, that the heterogeneous composition of *Cavalierius* was sufficiently evaded, and all the advantages of his method retained.

29. But here again the same absurdity occurs, as before. For if by the infinitely little breadth of these parallelograms we are to understand, what these words literally import, that is, no breadth at all; then they cannot, any more than the lines of *Cavalierius*, compound a surface; and if they have any breadth, the right lines bounding them cannot coincide with a surface bounded by a curve line.

30. The followers of this new method grew bolder than the followers of *Cavalierius*; and having transformed his points, lines, and planes, into infinitely little lines, surfaces and solids, they pretended, they no longer compared together heterogeneous quantities, and insisted on their principles being now become genuine: but the mistakes, they frequently fell into, were a sufficient confutation of their boasts. For notwithstanding this new model, the same limitations and cautions were still necessary. For instance, this agreement between the inscribing figures and the curved spaces, to which they are adapted, is
only

only partial, and in applying their principles to propositions already determined by a juster method of reasoning, they easily perceived this defect: both in surfaces and solids it was evident at the first view, that the perimeters disagreed. But as no one instance can be given, where these indivisible or infinitely little parts do so compleatly coincide with the quantities, they are supposed to compound, as in every circumstance to be taken for them without producing erroneous conclusions; we find where a surer guide to correct their reasoning was wanting or disregarded, these figures were often imagined to agree, where they ought to have been supposed to differ.

31. We might produce numerous instances of such errors from the writings of modern computists. The most celebrated of these, *Leibnitz* and *John Bernoulli*, will furnish us with sufficient examples, from the single error of supposing infinitely small arches absolutely to coincide with their chords.

32. The first, in two dissertations, one on the resistance of fluids, and another on the motion of the heavenly bodies, has on this principle reasoned falsely concerning the lines intercepted between curves and their tangents, as has been observed by *Sir Isaac Newton* himself*. And *Bernoulli*, in a dissertation likewise on the resistance of fluids, has made the same mistake, and even insulted over *Sir Isaac Newton*, where he had by means of his juster method of reasoning avoided that error; and *Bernoulli* upon the same principles had erred formerly in a pretended solution of the problem concerning isoperimetrical curves†.

33. But the proceeding of *Monf. Parent* is so very extraordinary, that it deserves also to be mentioned. He has had the rashness to oppose erroneous deduc-

* Philosoph. Transf. N^o 342. p. 208. Or Comm. Epist. p. 42.

† Acta Erudit. An. 1706.

tions from this absurd principle to the most indubitable demonstrations of the great *Huygens*. It having been shewn by *Galileo*, that any heavy body will descend through the chord of a circle terminating at its lowest point in the same time, as it will fall through its perpendicular diameter; from this principle of indivisibles, that the arch and the chord do at last coincide, it was falsely concluded, that the time of the fall through the smallest arches must be equal to the time of the fall through the diameter. This contest between demonstration and error has been thought of such importance, that several elaborate dissertations have been published to shew by what means, the idolized doctrine of infinitely small quantities could produce fallacious conclusions*.

34. Thus it appears, that the doctrine of indivisibles contains an erroneous method of reasoning, and in consequence thereof, in every new subject, to which it shall be applied, is liable to fresh errors.

35. It is also manifest, that the great brevity it gave to demonstrations, arose entirely from the absurd attempt of comparing curvilinear spaces in the same direct manner, as right-lined figures can be compared; for in order to conclude directly the equality or proportion of such spaces no scruple was made of supposing, contrary to truth, that rectilinear figures capable of such direct comparison could adequately fill up the spaces in question; whereas the doctrine of exhaustions does not attempt from the equality or proportion of the inscribing or circumscribing figures to conclude directly the like proportions of these spaces, because those figures can never in reality be made equal to the spaces, they are adapted to. But as these figures may be made to differ from the spaces, to which they are adapted, by less than any space proposed, how minute soever; it shews, by a just

* See Memoires de l'Acad. des Sciences, 1722.

though

though indirect deduction, from these inscribing and circumscribing figures, that the spaces, whose equality is to be proved, can have no difference, and that the spaces, whose proportion is to be shewn, cannot have a different proportion from that assigned them.

36. But Sir *Isaac Newton*, by his doctrine of prime and ultimate ratios, has found out the proper medium, whereby to avoid the impossible notion of indivisibles on the one hand, and the length of exhaustions on the other. As it was Sir *Isaac Newton's* express design absolutely to free his method of reasoning from every part of the obscurity and inconsistent notions of indivisibles, and as all the objections raised against him have entirely been grounded on a suspicion, that he has not fully succeeded in that design; we thought the readiest method of vindicating this doctrine was to cast it into a form as remote as possible from any appearance of agreement with that absurd system.

37. For this purpose we defined the ultimate magnitude of any varying magnitude to be a fixt quantity, which the varying one can approach within any degree of nearness, and yet can never pass: And the ultimate ratio of any varying ratio to be a fixt ratio, which the varying one can approach with any degree of nearness, but yet can never pass.

38. These definitions thus premised, we demonstrated the two following propositions.

I. That when the varying magnitudes continue always in the same ratio, their ultimate magnitudes will be also in the same ratio.

II. That all the ultimate ratios of the same varying ratio are the same to each other.

39. In this form it was easy to explain the doctrine of prime and ultimate ratios without the use of any of those expressions, which had been misunderstood to have reference to the method of indivisibles.

But

But to make appear the identity of this doctrine, as by us expounded, with what Sir *Isaac Newton* has himself delivered; we premised a short representation of the true sense, in which we apprehended, his phraseology ought to be understood. As he founded his doctrine on the first lemma in the first section of his *Principia Philosophiæ*, in my discourse I have thus represented his meaning in that lemma, That in this method any fixt quantity or ratio, which some varying quantity or ratio by a continual augmentation or diminution shall perpetually approach, but never pass, is considered as the quantity or ratio, to which the varying one will at last or ultimately become equal; provided the varying quantity or ratio can be made in its approach to the other to differ from it less than by any difference how minute soever, that can be assigned. Consonant to this representation of Sir *Isaac Newton's* meaning, in the account given of my book, this lemma was thus interpreted; those quantities are to be esteemed ultimately equal, and those ratios ultimately the same, which are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference.

40. And to this interpretation the following remark was subjoined, That this lemma did not mean, or necessarily imply, that any point of time was assignable, wherein these varying magnitudes would become actually equal, or the ratios really the same; but only that no difference whatever could be named, which they should not pass.

41. This interpretation of Sir *Isaac Newton* is evidently conformable to our definitions.

42. A learned gentleman, who, concealed under the name of *Philalethes Cantabrigiensis*, had expressly entered into controversy with the author of the
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Analyst, though he allows the truth of our method of reasoning to be unquestionable; yet he thinks, we have in some measure deviated from the exact intention of Sir *Isaac Newton*. He has interpreted the forementioned lemma after a manner something different. But his interpretation does not ascribe to the word *given*, used by Sir *Isaac Newton* in this lemma, the true sense of that word in geometry, but supposes it to stand for *assignable*; whereas it properly signifies only, what is actually assigned. *Philaletbes* insinuates, that by our interpretation, and the forementioned remark upon it, Sir *Isaac Newton* is rendered obnoxious to the charge of first supposing, what he would prove, and with proving only what he has before supposed. But our interpretation cannot possibly mean less than this, that those quantities and ratios will have no last difference, which are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference. This is certainly no identical proposition, though its truth be very obvious; and Sir *Isaac Newton*'s demonstration of it is accordingly very short.

43. However I acknowledge, had Sir *Isaac Newton* here no other intention, but simply to prove so obvious a truth, a distinct proposition for this purpose only might perhaps have well been spared. But we must consider this proposition in another light. There are two ways, whereby good writers explain the use of terms they introduce: one is by expressly defining them; another, when, to avoid that formality, they convey the sense of such terms by their manner of using them. And to make appear, that Sir *Isaac Newton*, by the demonstration annexed to this lemma, has sufficiently evinced; in what sense the lemma itself must be understood, and at the

same time to prove what that sense is, it was shewn, that this demonstration is no less applicable to quantities, which only approach without limit to the ratio of equality, than it will agree to such quantities, as at last become actually equal. For this purpose this demonstration was applied to the ordinate of an hyperbola, compared with the same continued to the asymptote, which do approach without limit to the ratio of equality, though they never become actually equal. But as *Philaethes* has taken exception to this instance, not conceiving how to regulate this approach, so as to bound it within a finite time; without enquiring how far that limitation was necessary to our purpose, we shewed a method of adding this circumstance by causing a line to turn upon the center of the hyperbola, and pass with an equable motion from the diameter to the asymptote: for by supposing the forementioned ordinate continually to accompany the intersection of this line with the hyperbola, the whole motion here required will be brought within such a finite space of time, as he imagined necessary. And in this view it is equally manifest, that the ordinate, and its continuation, can never become equal, till they are both extended to infinity, and all idea of them is lost*.

44. By this we think it very evident, that Sir *Isaac Newton* has neither demonstrated the actual equality of all quantities capable of being brought under this lemma, nor that he intended so to do.

45. When-

* *Philaethes* not perceiving, how the demonstration of Sir *Isaac Newton's* first lemma could be applied to this case, Mr. *Robins* thus shewed it.

Here the hypothesis assumed is a property of the hyperbola commonly known, That the curve continually approaches its asymptote, as it is farther extended, so that by removing any ordinate farther and farther from the vertex, it will approach nearer and nearer in magnitude to the same continued to the asymptote, without limit. Now to prove the ultimate equality of

45. Whenever the quantities or ratios compared in this lemma are capable of an actual equality, they must really become so. But when they are incapable of such equality, the phrase of ultimately equal must of necessity be interpreted in a somewhat laxer sense; that is, as Sir *Isaac Newton* in the 71 proposition of the first book of his *Principia* expresses it, *pro æqualibus habeantur, are to be esteemed equal**, and means only, that such quantities or ratios approach without limit. Accordingly we find, that immediately after this lemma he uses the expressions *ultimo in ratione æqualitatis*, and *ultimo æquales*, as synonymous terms. However, as in every subject of this lemma all ultimate difference is excluded, the consequences drawn

of these two lines, let us say thus. If you deny it, let them be ultimately unequal, and let their difference be *D*; therefore they cannot approach to equality nearer than by the given difference *D*, contrary to the hypothesis. Thus is the demonstration of this lemma, without changing a single word, applied to the present case †. *Present State of the Republick of Letters for August 1736*, p. 100.

* *Philalethes* objected to this translation, *are to be esteemed equal*, and contended, that it ought to have been rendered, *let them be taken for equal*, or, *let them be esteemed equal*. To which Mr. *Robins* replied thus.

But why is *habeantur* the imperative mood? *Philalethes* was deceived by the preceding words, which direct certain constructions; but here, where a consequence is concluded from these constructions, the potential mood is required, in which, to use the learned *Linacer's* words, *indicatio est potentia, debitive, aliquando voluntatis*. How then is, *are to be esteemed equal*, a false translation? Is not that *pro æqualibus haberi debent*? Just as *non expectes, ut statim gratias agat, qui sanatur invitatus*, is expounded by this great grammarian *non debes expectare*. See his excellent treatise, *De emendata structura latini sermonis, lib. 1. De Modo*.

I have quoted *Linacer*, because he was the first, who gave the name of potential to this mood, when it bears any of the three significations here mentioned: but this form of the verb having all these senses is a point agreed among grammarians. See *Alvarez* and *Vossius*. *Present State of the Republick of Letters for August 1736*, p. 102.

† Review, § 20.

from it are equally just and perspicuous, whether the quantities do or do not become actually equal, and the ratios actually coincident. And this restriction of the sense of this lemma is absolutely necessary to be attended to in this doctrine; because Sir *Isaac Newton* himself has applied it to quantities and ratios incapable of an actual equality or agreement.

46. In the account of our Discourse, the lemma immediately following, where parallelograms are inscribed, and others circumscribed to a curvilinear space, was produced as an example of this. It was there also observed, that vanishing quantities may never actually have that proportion, which, according to this lemma, is said ultimately to belong to them.

47. In this second lemma Sir *Isaac Newton* directs, that the number of these parallelograms should be augmented *in infinitum*. This must not be interpreted, till the number become infinitely great: for this is the express language of indivisibles. We render the words *in infinitum*, *endlessly*, and perform, what is here directed, by that simple and obvious method practised by the ancient geometers, of continually subdividing the base of the curve *. And it

* *Philalethes* thus animadverted on this place.

I apprehend our present enquiry is, not how Mr. *Robins* performs what is here directed; but how Sir *Isaac Newton* intended it should be performed.

There are two ways, which we may conceive the base of the curve to be continually subdivided. One is, the method practised by *Euclid* and the other *ancient Geometers*, which consists in continually repeating the operation directed in the tenth proposition of the first book, or in the tenth proposition of the sixth book of the Elements.

To which animadversion Mr. *Robins* made this reply.

Mr. *Robins* thinks himself directed by the words of Sir *Isaac Newton* to make the subdivision in the manner here proposed. *Philalethes*, in not understanding this place, confirms Mr. *Robins* in his opinion, how needful a knowledge in the ancients is to
qualify

it is manifest, that such subdivision can never be actually finished. *Philalethes* on the other hand endeavours to describe a complex kind of motion, whereby he apprehends this multiplication of the parallelograms can be brought to a period. But as the inscription and circumscription of these figures require, that the bases of those parallelograms be constantly equal, and each some aliquot part of the whole base, any such description by continued motion is necessarily excluded, as has been already observed *. *Philalethes* charges this with being too hasty an assertion: because in another lemma, namely the third, parallelograms are supposed to be described to a curve, whose bases should not be equal. But it was not asserted, that Sir *Isaac Newton* had supposed this equality in all the propositions, wherein he may have had occasion to consider parallelograms described to curves; but that it was constantly and always to take place in this second lemma; which being a distinct and separate proposition, must have a demonstration compleat within itself.

48. However, should we so far depart from Sir *Isaac Newton*, as to admit of this complex kind of motion, what idea can we form of the inscribed or circumscribed figure, to which we are at last

qualify a person for understanding either Sir *Isaac Newton* or himself. But however *Philalethes* has endeavoured to shew his knowledge in the ancients by quoting two propositions from *Euclid's Elements*; one of which teaches, how to divide a line in the same proportion as some other line is divided, the other shews, that by taking from any quantity more than half, and from the remainder more than half continually, the residue may be reduced within any degree of smallness. How much these propositions are to the purpose of inscribing and circumscribing parallelograms to a curvilinear space, let *Philalethes* shew. When *Philalethes* has gone further in the ancients than the *Elements* of *Euclid*, he will be better able to comprehend Mr. *Robins's* meaning, and judge upon the point in question. *Present State of the Republick of Letters* for August 1736, p. 107.

* Review, § 23.

actually to arrive, which with any propriety of speech is to be stiled equal to the curve?

49. Sir *Isaac Newton* in the corollaries annexed to the third lemma expressly declares, there is no ultimate sum of these parallelograms, nor no ultimate figure compounded of them, distinct from the very curve itself. *Philaletbes* himself acknowledges this. Though how far this concession agrees with the rest of his opinion, will be best understood hereafter. But at present we shall examine more particularly Sir *Isaac Newton's* meaning in the corollaries now mentioned.

50. The treatise of Sir *Isaac Newton*, in which this doctrine is delivered, is written throughout with that degree of brevity, as occasions a general complaint of the difficulties attending the study of it. And this conciseness is no where perhaps more remarkable than in the section now under consideration; insomuch, that it often requires careful attention to discover the exact meaning, and full force of the expressions. More than once Sir *Isaac Newton*, to convey his intention the easier to those, who had been accustomed to the method of indivisibles, has introduced some expressions analogous to the phraseology of that doctrine, when the brevity, he had prescribed to himself, occasioned his not giving express notice of it. Of this kind we must reckon the conclusion of the following passage. *Ultimæ rationes illæ, quibuscum quantitates evanescent, revera non sunt rationes quantitatum ultimarum, sed limites ad quos quantitatum sine limite decrefcentium rationes semper appropinquant; et quas propius assequi possunt quam pro data quavis differentia, nunquam vero transgredi, neque prius attingere quam quantitates diminuuntur in infinitum* *. The last words of this passage mean in reality no more, than that the quantities will never have the

* Newtoni Princip. Lib. 1. Lem. xi. in Schol.

ratio mentioned; and the only reason to be assigned, why Sir *Isaac Newton* expressed this in the manner he has done, is, that he addresses himself to those, who had been accustomed to indivisibles, and accommodates himself to their language. The expression in the first of the corollaries before-mentioned is evidently adapted to the same design. The *ultima summa* there mentioned, in strict propriety of speech, has no kind of meaning, for it is really infinite. His intention could only be here to signify, that what in the language of indivisibles might be called the last sum of these parallelograms, or the figures supposed in that method to be composed of such infinite number of parallelograms, or other right-lined figures, is really nothing distinct from the curve itself.

51. To assert that any collection of these inscribed or circumscribed parallelograms can ever become actually equal to the curve, is certainly an impropriety of speech; for equality can properly subsist only between figures distinct from each other. Such expressions therefore are so far from giving any additional advantage to this method of reasoning, that they can only tend to confound this method with indivisibles. For we have already shewn, that the essence of indivisibles consists in endeavouring to represent to the mind such inscribed or circumscribed figure, as actually subsisting, equal to the curve. That method does not merely depend on a particular set of exceptionable expressions; but however the phraseology be varied, yet while the same mode of thinking is attempted, we are still involved in that erroneous doctrine. Whatever state of the inscribed or circumscribed figure is supposed previous to this equality, or by whatever changes it is conceived to degenerate into its imagined last form: yet as long as we fancy ourselves capable of seeing directly in this last form those properties, which these figures had before, we are still immersed in the

the doctrine of indivisibles. It is certainly therefore an advantage to this method, to seclude from it any expressions leading towards such faulty conceptions.

52. But *Philalethes* seems apprehensive of weakening the force of Sir *Isaac Newton's* demonstrations by this means, when he tells us, that Sir *Isaac Newton* contents not himself with any the most near approximations, but carries his demonstrations to the utmost accuracy and geometric rigor; accordingly every one of the examples he has given in the lemmata of the first section, are of such quantities and ratios, as do actually arrive at their respective limits. Does *Philalethes* here suppose the truth of Sir *Isaac Newton's* demonstrations to depend on this actual equality of the variable quantity and its limit? He confesses our demonstrations to be just, which do not suppose this actual equality.

53. He also says, that *the supposing this actual equality seems greatly to exceed the method of the ancients in perspicuity as well as in the conciseness of its demonstrations.*

54. That this method should be more perspicuous is impossible, the method of the ancients being perfect in that respect. Certainly there are not in their method, what *Philalethes* (though I think without reason) insinuates of this, any demonstrated truths, that must be owned, though we do not perfectly see every step, by which the thing is brought about. That it exceeds the method of the ancients in conciseness is true; but that is not occasioned by this supposed actual equality of the variable quantity and its ultimate; since, as we have shewn this to be at the best but a superfluous circumstance, the supposing it necessary is so far from promoting conciseness, that it adds to the length of this doctrine, by obliging us to labour in every case after some idea of motion however intricate, whereby to represent to our minds this actual equality. In the case we have been considering, the motion proposed for forming these parallelograms is sufficiently intricate. If it were convenient

ent to make use of a polygon described within or without a curve, as in the first proposition of the second section of the first book of the *Principia*, another kind of motion must be accommodated to that case. And so for every variation of these inscriptions and circumscriptions we must strain our imagination for some involved and perplexed kind of motion applicable to each *.

55. The sum of what, we have been saying, amounts to this, that in our interpretation of Sir *Isaac Newton*, no subtle inquiry after means to bring about an actual equality between the curve and the inscribed and circumscribed figures is at all necessary; which is affixing intricate circumstances to this method, no way necessary to the truth or clearness of

* On this passage *Philaethes* thus observes.

However, since Mr. *Robins* is pleased to talk so much about *straining our imagination for some involved and perplexed kind of motion*, let us see, if we cannot find some plain and easy way, of representing to the imagination, that actual equality, at which the inscribed and circumscribed figures will arrive with each other, and with the curvilinear figure, at the expiration of the finite time.

Accordingly *Philaethes* attempted to represent to the imagination, what he calls the actual equality, at which the inscribed and circumscribed figures will arrive with each other, and with the curvilinear figure, at the expiration of the finite time.

To this Mr. *Robins* answered, Is this producing any continued motion for the purpose, Mr. *Robins* speaks of?

However, *Philaethes*, in executing his own design imagined two curves to be described, whose ordinates might express continually the proportion between the inscribed and circumscribed parallelograms in question; by the intersection or concurrence of which curves it might be found, when the inscribed and circumscribed figures become equal. But though each of these lines are drawn by *Philaethes* in his figure, as simple curves; yet in reality they are each compounded of an endless number of portions of as many different curves combined together. This, adds Mr. *Robins*, is a specimen of *Philaethes*'s skill in the common algebra of curve lines, to give us an equation expressing the nature of a single curve, one which in reality includes an infinite series. *Appendix to the Present State of the Republic of Letters for September 1736*, p. 1.

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the demonstrations. For this actual equality can scarce be represented to the mind, but in such complex, not to say confused ideas, that will bring us upon the very borders of indivisibles, and render us perpetually obnoxious to the absurdities of that doctrine. It is therefore without question an advantageous representation of this method, to free it from every perplexity of this kind. And we have shewn our interpretation, which thus removes this doctrine quite beyond the reach of every objection, that has hitherto been levelled against it, to be conformable to the most proper signification of Sir *Isaac Newton's* words, both in his second lemma, where those figures are considered, and also in the first, where the principles of this method are established.

56. And our interpretation of this first lemma is still more abundantly necessary in applying this lemma to what, Sir *Isaac Newton* calls vanishing quantities. For as these quantities are supposed continually to diminish, and by that means to have their proportions varied; nothing is more evident, than that their diminution will never bring them actually to bear that ratio to one another, with which in this lemma the ratio of these quantities is said to become ultimately the same.

57. This will most evidently appear by Sir *Isaac Newton's* words, *Ultimæ rationes illæ, quibuscum quantitates evanescunt, revera non sunt rationes quantitatum ultimarum, sed limites, ad quos quantitatum sine limite decrefcentium rationes semper appropinquant; & quas proprius assequi possunt quam pro data quavis differentia, nunquam vero transgredi, neque prius attingere, quam quantitates diminuuntur in infinitum. Res clarius intelligetur in infinite magnis. Si quantitates duæ, quarum data est differentia, augeantur in infinitum, dabitur harum ultima ratio, nimirum ratio æqualitatis; nec tamen ideo dabuntur quantitates ultimæ, seu maximæ, quarum ista est ratio. In sequentibus igitur, si quando facili*
rerum

*rerum conceptui consulens, dixero quantitates quam minimas, vel evanescentes, ved ultimas; cave intelligas quantitates magnitudine determinatas, sed cogita semper diminuendas sine limite *.*

58. Here it is expressly declared, that these *ultimæ rationes*, quibuscum *quantitates evanescent*, are not *rationes quantitatum ultimarum*; but only limits, to which the ratios of these quantities, which themselves decrease without any limit, continually approach; and to which these ratios can come within any difference, that may be given, but never pass, nor even reach those limits, before the quantities are diminished to nothing. To explain this more distinctly, and to prevent, as much as possible, his reader from seeking after any state or condition, at which these quantities can actually arrive, wherein to be the subjects of this proportion; he draws a parallel between this case, and the case of quantities supposed to augment without end. For says he, such quantities may have an ultimate ratio, though here will not be any last quantities as the subjects of that ratio.

59. *Philaletbes* is very unwilling to allow this intended for an exact parallel. But Sir *Isaac Newton* expressly affirms, his intention of introducing it was to render more clear the thing, he had been immediately speaking of, that is, the nature of the ultimate ratios of quantities decreasing without limit.

60. *Philaletbes* indeed asserts a difference between the two cases. In vanishing quantities he asserts their ratio actually to come up to (*atingere*) their limit; but in the other not. It is true the quantities in one case may be reduced absolutely to nothing, but in the other can never be extended to an infinite magnitude. Yet in both cases the quantities are equally incapable of being converted by the variation ascribed to them into any condition, wherein they will be the

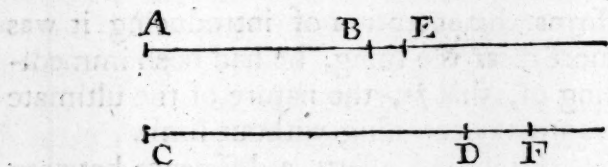
* Newtoni Princip. Philos. Lib. 1. Lem. xi. in Schol.

subjects

subjects of that ratio, which is called their ultimate. Nay, Sir *Isaac Newton* has been so particularly careful, lest any of his readers, from brief expressions, which they would afterwards find in this book, should imagine, he in the least favoured the attempt of indivisibles, to pursue such quantities to a diminution actually infinite, here further adds this express caution, that by whatever name he might hereafter denote these vanishing quantities, they were never to be considered as determinate, but as variable ones, diminishing without limit; consonant to what he said before, *Nolim indivisibilia, sed evanescentia divisibilia intelligi* *.

61. This interpretation of Sir *Isaac Newton*, so expressly conformable to his own words, at once dissipates all the objections, the author of the *Analyst* has raised against the demonstrations, whereby Sir *Isaac Newton* proves the operations in his method of fluxions, though condemned with so much freedom for fallacious and inconclusive.

62. The form of these demonstrations may be represented thus.



63. Let the two points B and D, one tracing out the line AB, and the other the line CD, be supposed to set forward together from A and C, and to arrive in the same time at B and D. Now it is required, from the given relation between these described lines AB and CD, in all the correspondent magnitudes of those lines, to determine the proportion of the velocities of the points at B and D; that is, to determine

* Newton. Princip. Phil. *ibid*.

the proportion of the fluxions of those lines AB and CD from the known relation of the flowing lines.

64. Suppose these points advance to E and F in the same time. Then if their velocities are always in the same proportion, the augments BE and DF will be in the proportion of those velocities. But if one of the points, suppose D, is more accelerated than the other B; then the ratio of DF to BE will be a proportion greater than that of the velocities: but the smaller BE and DF are taken, the nearer will the proportion of these spaces DF and BE approach to the proportion of the velocities at D and B; and the difference between these two proportions may be diminished in any degree whatever by a sufficient diminution of the augments DF and BE. Then the ultimate proportion of the decreasing quantities DF and BE will be the true proportion of the velocities, or of the fluxions at D and B.

65. This is expressly the method, by which Sir Isaac Newton determines the proportion of the fluxions of different magnitudes in all cases. Having first compared such contemporaneous augments as have a finite, that is, a real magnitude; then he supposes these augments continually to diminish, and having determined the nearest proportion, to which they constantly tend during their diminution, he assigns this as the true proportion of the velocities or fluxions.

66. Sir Isaac Newton describes the process, we have here explained, by these words: † *Evanescant jam augmenta illa & eorum ultima ratio erit, &c.*

67. Now this passage is thus translated and commented on by the author of the *Analyst*, *Let these increments vanish, i. e. let the increments be nothing;* and from hence he takes great pains to shew the absurdity of comparing together, and assigning the pro-

† Introd. ad Quad. Curv.

portion of quantities, after they are supposed to be destroyed.

68. But here he commits a twofold error: first in imagining, that the operation, which the increments are by Sir *Isaac Newton* supposed to undergo in order to have their ultimate proportion assigned, and which he describes by the verb *evanescent*, is confined to that point of time only, at which the increments are actually gone and abolished; and, secondly, in imagining, that by the ultimate ratio of varying quantities is meant a ratio, that those quantities do at some time or other exist under.

69. As to the first supposition, when the points E and F are conceived to move backwards, till they arrive at B and D, the diminishing of the augments BE and DF, as well as their abolition at last in the points B and D effected by this means, is by Sir *Isaac Newton* comprehended under the general description of *evanescent*, let them vanish; as is most evident, not only by the passage above quoted from Sir *Isaac Newton's* introduction to his treatise of Quadratures, where he says his analysis investigated *finitarum nascentium vel evanescentium rationes primas vel ultimas*, but also by the words since produced, that *the vanishing quantities by him considered are divisible, and not determinate, but continually diminishing*.

70. Since therefore these vanishing quantities are expressly declared by Sir *Isaac Newton* to be finite and variable; his expression in this place must be understood to relate to the whole time they are vanishing. And his words are free from any impropriety; for the term vanishing is daily applied to objects during the time of their disappearing, before they are actually out of sight, absolutely signifying no more than going to vanish. Just as we say that the sun is setting in the most limited signification of that word, as soon as its under limb touches the horizon; and as soon as ever the sun is quite out of sight, it is no longer

longer setting, but actually set : so these quantities, being of a finite, that is, of a real magnitude, do not vanish instantaneously, but with the utmost propriety may be said to be vanishing all the time they are undergoing the diminution ascribed to them.

71. The second error of the author of the *Analyst*, that of supposing the ultimate ratio of varying quantities to be a ratio, which these quantities must some time or other exist under, we have fully shewn to be contrary to Sir *Isaac Newton's* express declaration.

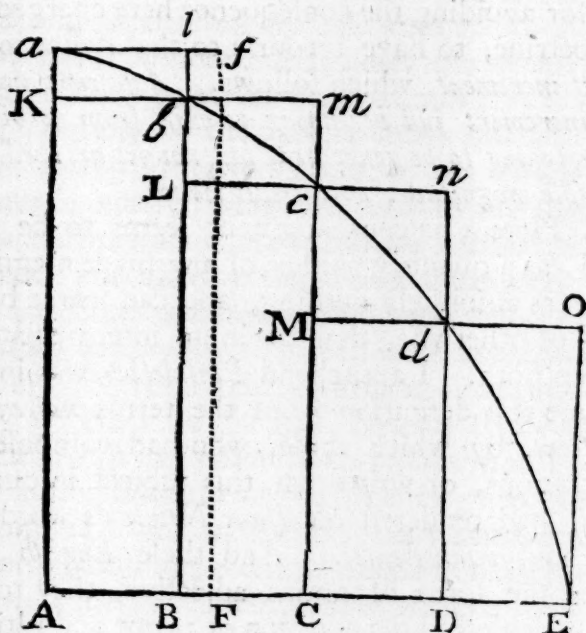
72. Upon this mistake is grounded the charge of indivisibles being unavoidably supposed in this doctrine, when he says, no quantity can be admitted as a medium between a finite quantity and nothing, without admitting infinitesimals. By what has been above said, it appears, that *Philalethes* had no necessity, for avoiding the consequence here charged upon the doctrine, to have recourse to that definition of a *nascent increment*, which follows. *A nascent increment is an increment just beginning to exist from nothing, or just beginning to be generated, but not yet arrived at any assignable magnitude, how small soever.*

73. Here a nascent augment seems to be represented, as a quantity neither of any finite magnitude, nor yet as absolutely nothing, and can scarce be conceived of otherwise, than as in an intermediate state between both. I apprehend *Philalethes* was induced to frame this definition from the terms *nascent* and *evanescent*, by which those, who had composed demonstrations, or writ upon this subject in our language, had rendered Sir *Isaac Newton's* words *nascentes* and *evanescentes*. And these *English* words bearing the form of nouns adjective, they too frequently neglected the addition of prime and ultimate, necessary to render the sense compleat in expressing the ratio, which is the limit to the ratios of these quantities. We find Sir *Isaac Newton* so cautious in this particular, that in the account of the *Commer-*
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cium Epistolicum, published in the *Philosophical Transactions*, though written by him in *English*, he retains the *Latin* expressions. Since Sir *Isaac Newton* intended by *quantitates nascentes* and *evanescentes* finite and indeterminate quantities capable of bearing different proportions, the term prime or ultimate is absolutely necessary to express that ratio, which is the limit of those different ones.

74. From this inadvertent use of the words *nascent* and *evanescent* we expressed a dislike to them in our discourse.

75. But now, to sum up the whole of what we have said upon this head, since Sir *Isaac Newton* has expressly told us, that the quantities, he calls *nascentes* and *evanescentes*, are by him always considered



as finite quantities; that ratio called their prime or ultimate cannot be the ratio, which those quantities themselves at any time must actually have. Our interpretation

terpretation therefore of the lemma so often mentioned is absolutely necessary in applying it to these quantities. And as we have shewn no different sense to be required in any other subject of this doctrine; so our representation of Sir *Isaac Newton's* mind has not only been proved to be conformable to the genuine meaning of his words, but is also perfectly consistent with itself: whereas we must confess ourselves at a loss to reconcile *Philalethes* with himself in the acknowledgment he makes, that the ultimate form even of the perimeter of the inscribed figure in Sir *Isaac Newton's* second and third lemmas is no other than the curve itself; that is, in each triangle aKb , bLc , cMd , dDE , the rectilinear sides aK , Kb , bL , Lc , cM , Md , dD , DE , vanish into the curve itself: if at the same time he supposes evanescent quantities subsisting at each point of the curve, which can be the subjects of the proportions, between the ordinate, tangent, and subtangent.

76. We shall now proceed to consider, what Sir *Isaac Newton* has called the *momenta* of quantities. This term was used very early by him. In or before the year 1669*, he drew up a short discourse *de Analyfi per æquationes numero terminorum infinitas*. Here the word moment frequently occurs. He has told us *this tract teaches how to resolve finite equations into infinite ones, and how by the method of moments to apply equations both finite and infinite to the solution of problems*†. He says, that *he there called the moment of a line a point in the sense of Cavalerius, and the moment of an area a line in the same sense*‡. The passage in the book, to which this relates, is as follows. *Nec vereor loqui de unitate in punctis, five lineis infinite*

* *Philos. Transf.* No. 342. p. 204. Or *Comm. Epist.* p. 36.

37.

† *Philos. Transf.* *Ibid.* p. 176. Or *Comm. Epist.* p. 5.

‡ *Philos. Transf.* *Ibid.* p. 178. Or *Comm. Epist.* p. 7.

parvis, siquidem proportionibus ibi jam contemplantur geometriæ, dum utuntur methodis indivisibilium||. And he has told us, from the moments of time he gave the name of moments to the momentaneous increases, or infinitely small parts of the abscissa and area generated in moments of time**. He says, Leibnitz bath no symbols of fluxions in his method, but used the symbols of moments or differences dx , dy , dz ††. All this is fuitable to the doctrine of indivisibles. He likewise tells us, because we have no ideas of infinitely little quantities, he introduced fluxions into his method, that it might proceed by finite quantities as much as possible|||. Hence it appears, he had not at the first discovered his doctrine of prime and ultimate ratios, which entirely rejects indivisibles, or infinitely little quantities; but at length falling upon it, he founded his method [of fluxions] on the primæ quantitatum nascentium rationes, which have a being in geometry, whilst indivisibles, upon which the differential method is founded, have no being either in geometry, or in nature*. Accordingly he tells us, When he is demonstrating any proposition, he uses the letter o for a finite moment of time, or of its exponent, or of any quantity flowing uniformly, and performs the whole calculation by the geometry of the ancients in finite figures or schemes without any approximation: and so soon as the calculation is at an end, and the equation is reduced, he supposes, that the moment o decreases in infinitum, and vanishes. But when he is not demonstrating, but only investigating a proposition, for making dispatch he supposes the moment o to be infinitely little, and forbears to write it down, and uses all manner of approximations, which he

|| Comm. Epist. p. 85.

** Philos. Transf. Ibid. p. 178. Or Comm. Epist. p. 7.

†† Philos. Transf. Ibid. p. 205. Or Comm. Epist. p. 37, 38.

||| Philos. Transf. Ibid. p. 205. Or Comm. Epist. p. 38.

* Philos. Transf. Ibid. p. 205. Or Comm. Epist. p. 38.

conceives

conceives will produce no error in the conclusion*. Here Sir *Isaac Newton* declares, he was wont to use the word moment in two senses; examples of both which he then mentions. And it is observable in his rule for finding the relation of fluxions, as published out of his old papers by Dr. *Wallis* in 1693, the word moment is used in the sense of indivisibles; but when he came to give that rule himself in his book of *Quadratures* first printed in 1704, he used that word in the other sense†.

77. Before he had published any thing on these subjects, he thought fit for the sake of brevity to in-

* *Philos. Transf. Ibid. p. 179. Or Comm. Epist. p. 9.*

† But *Philalethes* contended, that by the expression *infinite parvis*, once only used in Sir *Isaac Newton's Analysis*, he did not mean indivisibles; because in that work the expressions, *in infinitum diminui et evanescere, sive esse nihil; continuo diminuaturnec tandem evanescent; diminuetur donec evanescent; continuo decrescit donec tandem penitus evanescent*: were all conformable to Sir *Isaac Newton's* constant doctrine, and peculiarly his own, and never used by any writers upon indivisibles. To which weak argument Mr. *Robins* made the following reply.

Here *Philalethes* is too hasty; for such expressions are to be found in Dr. *Wallis*. Sir *Isaac Newton's* doctrine of prime and ultimate ratios does not depend merely upon these words. It would be tedious to refer to all the places of Dr. *Wallis*, where such expressions as these occur. It is sufficient to name the two following. In the *Arithmetick of Infinites*, *propp. 20, 40*, are these words, *excessus ille, si in infinitum procedatur, prorsus evaniturus est. Huygens* in his illustration of *Fermat's* method of drawing tangents, though he proceeds undoubtedly upon the principles of indivisibles, yet has made use of the word *evanescens* in the following passage, *Nam termini, &c. quantitates infinite parvas, sive omnino evanescentes continebant**. It is certainly very easy to see, what Sir *Isaac Newton* meant by the words *infinite parvis* in the *Analysis*; because he tells us expressly, that he used them in imitation of those, who used indivisibles. *Nec vereor loqui de unitate in punctis, sive lineis infinite parvis, siquidem proportionibus ibi jam contemplantur geometræ, dum utuntur methodis indivisibilium.* Appendix to the Present State of the Republick of Letters for September 1736, p. 17.

* *Divers Ouvrages de Mathématique et de Physique par Mess. de l'Académie Royale des Sciences, à Paris 1693, p. 332.*

roduce this term *moment* in the second book of his *Principia Philosophiæ*. As the geometers of his time had been much accustomed to indivisibles, he did not scruple there to describe moments according to the sense of that doctrine, as he had done formerly, to be *incrementa vel decrementa momentanea* †. As in another place of that treatise he acknowledges his using several expressions favouring indivisibles, but at the same time shews how that idea may be corrected, when such expressions occur ‖; so likewise here he does the like: he shews how to correct the idea arising from this description of moments. He says, *you must never consider their magnitudes, but their prime ratio*. He adds, *it would come to the same thing, if instead of these moments you used the velocities of increase or decrease of quantities, which he is wont to call fluxions, or if you used any finite quantities proportional to these fluxions*.

78. Notwithstanding all this caution of Sir *Isaac Newton*, he has not escaped being censured. I therefore endeavoured in my discourse to clear up this affair of the moment, not thereby to vindicate the genuineness of Sir *Isaac Newton's* methods of fluxions, and of prime and ultimate ratios, which I had before sufficiently shewn to be accurate, and did not in the least depend on the interpretation of the word moment; but to make appear, as on this head there had been raised great clamor and boasting, that it was without any manner of foundation. To this end in my discourse I gave a description of moments suitable to the doctrine of prime and ultimate ratios; and since Sir *Isaac Newton's* demonstration of the moment of a rectangle had been attempted to be exploded, though it is most accurate as well as brief, this is likewise explained; and is also more fully enlarged upon in the account of my book.

† Lemm. II.

‖ Lib. I. Lemm. XI. in Schol.

79. The mistakes, that have here arisen, were occasioned by not sufficiently attending to Sir *Isaac Newton's* last mentioned caution. From thence it will appear, in calling $\frac{1}{2}a$ and $\frac{1}{2}b$ the halves of the moments of A and B, by a and b he meant finite quantities in the prime or ultimate ratio of the correspondent increments or decrements of A and B.

80. Upon this principle, if the sides of a rectangle, which are denoted by A and B, be augmented and diminished by half such lines expressed by a and b , as shall be in the ultimate ratio of the increments or decrements of the sides A and B, generated in equal portions of time; the difference ($aB + bA$) of such rectangles, as are contained by the sides A and B thus augmented and diminished, will express the momentum of the original rectangle.

81. The exception to the demonstration, *Philalethes* has given, of the method for finding the momentum of a rectangle is, that as it demonstrates too much, it must of necessity be inconclusive.

82. He has endeavoured to prove, that the moment of the rectangle is an arithmetick mean proportional between the contemporaneous increment and decrement of the same rectangle.

83. But it has been shewn, that this is only true, when the sides augment in the same constant proportion.

84. Consequently the supposed demonstration of *Philalethes* must be defective; for there is no part of it, but the conclusion, that contains any restriction to this particular case.

85. *Philalethes* says, that Sir *Isaac Newton* never admitted of indivisibles, nor of quantities infinitely small, conceived as actually existing in a fixed determinate and invariable state.

86. That Sir *Isaac Newton* has made use of indivisibles in the very sense of *Cavalierius*, and that the

doctrine of moments was originally founded on them, we have already proved from his own words.

87. We are also told, *that Sir Isaac Newton in the lemma, where he determines the moments, amongst other methods of conceiving them considers these moments as the differences of Leibnitz, or as Philalethes afterwards explains it, as finite quantities exceedingly small.* But this is directly contrary to Sir Isaac Newton's description, who, speaking of these *momenta*, as *incrementa momentanea*, in the sense of indivisibles, says, *particulæ finitæ non sunt momenta*. Afterwards indeed he adds a caution, whereby we may understand the signification of the word moment in the true sense of his method of prime and ultimate ratios; that is, that these *momenta* may be expressed by finite quantities, not confined to be exceedingly small, but of any magnitude, provided they were in the prime or ultimate ratios of their corresponding increments or decrements.

88. And it is afterwards said, *that the course taken by Sir Isaac Newton to find the finite difference of variable quantities, though not rigorously geometrical in the higher cases, yet approaches nearer to geometric rigour than the method of Leibnitz.*

89. Now I say, that were these differences in this sense considered as finite small quantities; however, Sir Isaac Newton's computation might come nearer the value of any quantity sought after, than that of Leibnitz; yet considered as a medium of demonstration to determine the absolute value of such quantity, both will be totally, and therefore equally, void of geometric rigour.

90. It is said, *that in the first case of this lemma Sir Isaac Newton is naturally to be understood as considering the sides of the rectangle to flow either uniformly or proportionally.*

91. I say this cannot be the natural interpretation of that case, because it is immediately quoted to
prove

prove the second case, where the augmentation is confessedly different. Nor can there be any reason assigned to shew, why it should be thus understood : for Sir *Isaac Newton* has computed the moment of the rectangle, not by supposing the sides increased and diminished by their respective increments and decrements generated in equal portions of time, but by finite lines expressing half their correspondent moments, as we explained it above ; so that his determination is by this means general, and according to the utmost geometric rigour.

92. I hope, I have here not only shewn, that the account, I lately published, of Sir *Isaac Newton's* doctrines of fluxions, and of prime and ultimate ratios is entirely conformable to the sense of that great man ; but have also placed them in such a light, as the objections, that have been raised against them, will at once appear to proceed from misconceptions and misrepresentations of the subjects. Sir *Isaac Newton* has been charged with having given various and inconsistent accounts of his methods, and been represented as struggling with insuperable difficulties, and imposing on his followers. How little reason there is for all these imputations will be manifest from the following considerations.

93. Sir *Isaac Newton* being very young at the beginning of his mathematical studies, discovers a very extensive and compendious method of calculation, which he readily applied to the finding the *maxima* and *minima*, drawing tangents, determining the curvature of curves, squaring curvilinear surfaces, and to other problems of the like sort. About the year 1665, because, as he says, we have no ideas of indivisibles, or infinitely little quantities, he introduced fluxions into his calculations, that he might proceed without indivisibles, as much as possible. But in determining the proportions of these fluxions he still allowed himself some use of infinitely little quantities.

quantities. No doubt, but upon reading the ancients he from thence would have been enabled to have demonstrated the proportions of fluxions according to their accurate methods; for he did much more, by finding out one of his own, more compendious than theirs, and equally geometrical. This served not only to demonstrate the proportions of fluxions, but was applicable to the synthetic demonstration of all propositions relating to curves. When he discovered this method of prime and ultimate ratios, we cannot certainly know. We are sure he had part of it in 1669, on account of a demonstration added at the end of his *Analysis per æquationes, &c.* which was sent at that time by Dr. Barrow to Mr. Collins. But most probably he had not then compleated this method, since in the *Lectures* he read the same year at Cambridge on his admirable discoveries in opticks, he used indivisibles in his demonstrations*.

94. It was in 1686 he first disclosed his doctrine of prime and ultimate ratios in his immortal work the *Mathematical Principles of Natural Philosophy*. It

* *Philalethes* affirming Sir Isaac Newton never used indivisibles in his demonstrations, Mr. *Robins* thus replied.

Whoever has read Sir Isaac Newton's *Lectiones Opticæ*, and will deny, that he has at any time made use of indivisibles, must be very much a stranger to that doctrine, and to the style of those writers, who followed it; but I shall set down two passages, where Sir Isaac Newton owns, that he used the phrases of indivisibles in the sense then generally understood. In page 98. *Concipias itaque arcum bc in æquales et indefinite multas partes dividi, et ejusmodi tot sumi, quæ minus quam una parte (hoc est, indefinite parum) differunt ab arcu cd, atque adeo ipsi pro more consueti censeantur æquales, &c.* Again in page 127. *Age NZ occurrentem CI in g, et, ut mos est, concipe infinite parvum arcum BN æqualem esse, &c.* Here the words *pro more consueti*, and *ut mos est*, plainly shew, that by the phrases *indefinite multas*, *indefinite parum*, and *infinite parvum* he meant the same as other writers had done. *Appendix to the present State of the Republic of Letters for Sept. 1736. pag. 19.*

is surprising with what modesty, and as it were fearfulness to offend such, as had been admirers of indivisibles, he introduced so excellent and truly geometrical method, by censuring the other in the softest manner. Though in answering the objections, that might be started against his own method, he evidently proves, he was fully apprised of the real imperfections of indivisibles, at the same time shewing a way to avoid them; yet he scarce condemns them himself, and frequently makes use of expressions peculiar to them, thinking it sufficient once for all to inform those, who did not approve of indivisibles, how to correct such expressions, and render them conformable to his method of prime and ultimate ratios.

95. In this treatise he but once mentions his doctrine of fluxions, and though what he says of them is short, yet it is very just. It seems, as if he took notice of them chiefly, that a cypher, he thought fit to explain relating to them, might be understood. He here indeed demonstrates synthetically, and very accurately the foundation of his method of calculation, which is common to the methods of fluxions, of prime and ultimate ratios, of moments, and of differentials.

96. In the year 1704. he published his book of Quadratures, a work worthy his profound genius. He had now sufficiently seen the abuses, that had been made of the doctrine of infinitely small quantities, in what was called the differential calculus. In the introduction to this book he delivers a very distinct account of his method of fluxions, and teaches how to find out their proportions by his method of prime and ultimate ratios; in order, as he says, to shew there was no occasion in the use of fluxions to introduce infinitely little quantities into geometry: but still with his usual modesty saying errors might be avoided in the other method, if we proceeded

proceeded cautiously. In all this plain narrative of matter of fact, there appears no inconsistency in Sir *Isaac Newton's* accounts of his methods, or the least shadow of having been ever puzzled or confounded in his ideas about them.

97. Hence it is very manifest, that as Sir *Isaac Newton* used at first indivisibles, so he soon corrected those faulty notions by his doctrine of fluxions, and afterwards by that of prime and ultimate ratios. And all the absurdity of expression, and all the inconsistency with himself charged on him by the author of the *Analyst*, arises wholly from misrepresentation. For example, it has been asserted, that there is as little sense in the phrase, a fluxion of a fluxion, as to speak of the velocity of a velocity *.. This objection supposes, that the simple word velocity can always be substituted in the room of the word fluxion. But by Sir *Isaac Newton's* description of the fluxions of magnitudes, it is evident, that the single words can never be used promiscuously: for the fluxion of any quantity is not the velocity of that quantity, but the velocity, wherewith it at all times augments or diminishes; for instance, the fluxion of a line is not the velocity, wherewith that line moves, but the velocity of the point, by whose motion the line is described. Therefore, first fluxions not being the velocities of their fluents, but the velocities, with which the fluents increase or diminish, the fluxion of a fluxion is not the velocity of the first fluxion, but the velocity or degree of swiftness, with which the first fluxion increases or diminishes. Again, because Sir *Isaac Newton* has said fluxions are in the first ratio of the *augmenta nascentia*; therefore fluxions, and what some are pleased to call nascent augments, are so absurdly confounded together, that the expressions of a fluxion of a fluxion and the nascent augment of

* Defense of Free-thinking in mathematicks, p. 24.

a nascent augment are represented as synonymous. Lastly, the description, Sir *Isaac Newton* has given, of moments for their use, who had been accustomed to the method of indivisibles, is set up as a standard to interpret his doctrines of fluxions and of prime and ultimate ratios; and the cautions he gave, in order that the term moment might be understood suitable to those doctrines, have been either neglected or misunderstood, and himself represented as imposing on his followers. And as Sir *Isaac Newton* from the demonstration, he has given, of the *momentum* of a rectangle, and of other more compound quantities deduces the *momentum* of a power, which *momentum* may be applied to determine the fluxion of such powers: he is thence charged with being unsatisfied with the truth of his demonstration, nay of exerting the utmost subtilty and skill, the greatness of his genius was capable of, in struggling with a difficulty imagined insuperable; for no other reason, than because he has given another more direct demonstration of the fluxion of powers in the introduction to his treatise of Quadratures, which differing a little in form from the other, is represented as grounded upon different principles; whereas in the demonstration of the *momentum* of a rectangle the moments of the sides are taken instead of their compleat increments, upon the very same foundation as the superfluous terms are rejected out of the increment of the power in the other demonstration. And this author thinks it very reasonable to suspect, that Sir *Isaac Newton* might not be fully persuaded of the truth of what, he has undertaken expressly to demonstrate; because he has happened to declare himself so cautious upon a certain point, as to decline the attempt of demonstrating it, though he was strongly persuaded of its truth*.

* Analyst, p. 27.

98. In the account of his book of Quadratures given in the *Acta Eruditorum* in 1708, there is an insinuation reflecting on the candour and honour of this most excellent person.

99. This occasioned old papers to be examined in order to vindicate his reputation; the result of which was published in a treatise entituled *Commercium Epistolicum D. J. Collins, &c.* This made it plainly appear, that he was the real inventor of these methods, we have been describing, and proved his rival *Leibnitz* in several particulars a most egregious plagiary.

100. In an account of this book written by Sir *Isaac Newton* himself, and often referred to in this dissertation, he without reserve discovered his dislike to indivisibles or infinitely little quantities, and being here obliged to compare his methods with that called *Leibnitz's* differential calculus; he absolutely denied this latter capable of demonstrating a proposition, because it was founded on indivisibles; which clearly proves, he lookt on no demonstration as valid, that was built on those absurd principles.

101. To conclude, I am sorry, that in any particulars relating to Sir *Isaac Newton's* doctrine of prime and ultimate ratios I should differ in sentiment from the learned *Philalethes*, and perhaps from several other excellent persons. As I learnt this doctrine solely from Sir *Isaac Newton's* Works; so the account, I have published of it, is agreeable to the opinion, I had ever entertained concerning it. Notwithstanding the terms of indivisibles, and other figurative expressions, that frequently occur in his writings, I always thought his true design very manifest. About many of these forms of speech he has given us a caution in order to prevent mistakes, declaring he used them to assist the imagination. But there are others of the like sort not expressly taken notice of, he perhaps believing no body liable to be deceived

deceived by their use. The ultimate ratio of vanishing quantities, the ratio with which quantities vanish, are in strict propriety of speech figurative expressions: nay, the last form of a figure, and the form, wherewith a figure vanishes, might be interpreted upon the foot of indivisibles. But here these phrases only signify the limits, to which the ratios of the vanishing quantities, and the forms of the changing figures approach within any degree of nearness without being ever able to arrive at them. As in Opticks, though the focus of rays after refraction is understood to denote the place, where they meet; yet the point on the axis of any spherical glass, assigned by optical writers for such focus, is the point, which only limits all the intersections of a pencil of rays with the axis after their refraction, and is so far from being the point, where any number of rays actually unite, that no individual ray, excepting that which passes in the axis itself, is supposed to pass through it. Thus the last form of a vanishing figure is not the form, which that changing figure will ever arrive at, or subsist under; but a form, to which it will approach within any degree of nearness possible to be assigned. To assert that a vanishing triangle, for instance, would ever exist under that form, which is called its last, would be, as the author of the *Analyst* expresses it, to imagine a triangle in a point. But this is not Sir *Isaac Newton's* meaning. These ultimate ratios, and last forms are the limits only of the continually varying ratios, and changing forms of the vanishing quantities and figures. And it is the whole purpose of this method, from these varying ratios and changing forms to find the fixt ratio and stable form, which is said to be the last of these varying ones.

102. This at present is my real opinion of this doctrine. I do not deny, but some expressions may be found in Sir *Isaac Newton's* works consistent with
another

another sense; and I am also very sensible, how frequently he accommodated himself to the way of writing of the mathematicians of his time, partly for assisting their imagination, and partly, perhaps, for fear he might seem to affect innovation. But I make no scruple of interpreting these expressions suitably to my representation of this doctrine; for otherwise I acknowledge myself totally incapable of reconciling this method of prime and ultimate ratios with the character, the author himself has given of it.

103. He prefers it to indivisibles, because, he says, they have no place in geometry, or in nature; and he often insists, that his method is conformable to the geometry of the ancients. But how can we pursue the variable and fleeting forms of the inscribing and circumscribing figures *in infinitum*, so that, when they should become equal to the curve, they may not totally withdraw themselves from the imagination, and all idea about them be lost? It is certain such refined metaphysical notions are not in the least analogous to that simplicity and clearness in the ideas, to which the ancient geometers ever confined themselves. However, the conception of vanishing quantities ever actually arriving at their ultimate proportion, has above been shewn to be unquestionably inconsistent with nature. Nay farther, I will not pretend on any other principles, than those I have set forth in my discourse, to defend Sir *Isaac Newton* from the consequences of the mistakes, he has been charged with: for the asserting, that the varying ratios of vanishing quantities, and the changing forms of vanishing figures would ever attain those ultimate ratios, and last forms, seems to me directly to impinge upon those principles, he has expressly declared to be absurd.

A D D I T I O N

B Y T H E

P U B L I S H E R.

104. **T**HE foregoing tracts of Mr. *Robins* not only fully explain Sir *Isaac Newton's* doctrines of fluxions and of prime and ultimate ratios; but also vindicate them as well against the misrepresentations of the author of the *Analyst* as those of *Philaethes Cantabrigiensis*, inasmuch that the former became silent; however the latter replied, and as in so doing he passed the bounds of good manners, Mr. *Robins* only made cursory remarks on the considerations *Philaethes* published. These remarks were printed in the Present State of the Republick of Letters for *August* 1736, and in an Appendix to that for *September* of the same year.

105. Some of these remarks I have already given as notes to the preceding dissertation: I shall here farther give the conclusions of them, as it contains a summary account of *Philaethes's* capital mistakes.

106. " This is all we think necessary to remark
 " upon these *considerations* of *Philaethes*, and we
 " trust our unprejudiced reader will not find many
 " of his exceptions to Mr. *Robins* unanswered; and
 " we further hope, that we have not allowed our-
 " selves in any warmth or freedom of expression,
 " which the present intemperate manner of *Phila-*
 " *ethes's*

“ *letbes*’s writing will not sufficiently excuse. We
 “ shall not here draw the usual inference from this
 “ change of behaviour in *Philaletbes*, that he is be-
 “ come doubtful of his own cause; for Mr. *Robins*
 “ rather desires his readers to compare impartially,
 “ without any bias the writings of *Philaletbes* with
 “ his own, and to give the preference, where they
 “ find the greatest weight of unmixed and undis-
 “ guised argument. But to remove a difficulty,
 “ which naturally lies in the way, I shall conclude
 “ this paper with examining, how it has come to
 “ pass, that *Philaletbes* and Mr. *Robins* should both
 “ carefully have studied Sir *Isaac Newton*, with a
 “ sincere intention of understanding him, and yet
 “ differ so much from one another.

107. “ I think it evidently appears from the paper
 “ of *Philaletbes*, we have been considering, that his
 “ reading in the mathematicks has been very much
 “ confined. Had he been acquainted with the an-
 “ cient writers, he could have been at no loss to un-
 “ derstand, what Mr. *Robins* meant by saying, that
 “ he described the parallelograms of the second lemma
 “ after the manner of the ancients by subdividing
 “ the base of the curve. He could never have
 “ thought, that Mr. *Robins* had there reference to
 “ two propositions of *Euclide*; in one of which
 “ *Euclide* had no view at all to demonstrations by
 “ exhaustions, and the other, though used by *Euclide*
 “ in one or two such demonstrations, is very unfit to
 “ be applied to the subject Mr. *Robins* mentions *.

108. “ Had *Philaletbes* been versed in the ancients,
 “ and in the latter writers, who have imitated them,
 “ he could have been at no loss about the true sense
 “ of *data quavis differentia* used by Sir *Isaac Newton*
 “ in his first lemma. For this expression is borrowed
 “ from the writers, that made use of exhaustions †.

* Dissertation, §. 47.

† Dissertation, §. 36.

109. " The first proposition of the tenth book
 " of the *Elements*, which is applied by *Euclide* both
 " in his comparing of circles, of pyramids of
 " equal altitudes, and in one or two propositions
 " more, is thus expressed. Two unequal magni-
 " tudes being proposed, if from the greater be
 " taken more than half, and from the residue more
 " than half, and so on, there will be left at length
 " a magnitude less than the lesser of the proposed
 " magnitudes. This is directly, as *Mr. Robins* has
 " represented it, first assigning a difference, accord-
 " ing to which the degree of approach is afterwards
 " to be regulated.

110. " *Archimedes* in his treatise on the *Sphere and*
 " *Cylinder* proposes to shew, when a circle and ano-
 " ther space are given, that it is possible to circum-
 " scribe a polygon, so that the excess of the poly-
 " gon above the circle shall be less than the space
 " given. In his book of *Conoids and Spheroides* it
 " is shewn in Prop. xxi, that any segment of a
 " conoide being given cut off by a plane perpendi-
 " cular to the axis, or any segment of a spheroide
 " not greater than half the spheroide cut off in like
 " manner, it is possible to inscribe a solid figure,
 " and to circumscribe another consisting of cylin-
 " ders, so that the circumscribed shall exceed the
 " inscribed by less than any solid magnitude, that
 " shall be given. And this he performs by a con-
 " tinual bisection of the axis, till a cylinder is found
 " less than the space, that should be given, by which
 " cylinder the inscribed and circumscribed figures
 " differ from each other. From this proposition
 " *Philalethes* may know, what *Mr. Robins* means,
 " when he speaks of describing the parallelograms in
 " *Sir Isaac Newton's* second lemma by continually
 " subdividing the base of the curve.

111. " After the same manner the following ex-
 " cellent writers express themselves.

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L

112. " *Fed.*

112. "*Fed. Commandinus de Centro Gravitatis solidorum, Prop. xi — ita ut circumscripta superet inscriptam magnitudine, quæ solida magnitudine sit minor. Ibid. Prop. xxviii — ita ut recta linea, quæ inter centrum gravitatis portionis et figuræ inscriptæ, vel circumscriptæ interjicitur, sit minor quamlibet recta linea proposita.*

113. "*Lucas Valerius de Centro Gr. solid. Lib. i. Prop. vi. — ita ut circumscripta superet inscriptam minori spatio quantacunque magnitudine proposita.*

114. "*Joannes della Faille de Centro Gr. Partium circuli et ellipsis, Prop. viii. — superet latus sectoris intervallo, quod minus sit quolibet dato. Id. Prop. xxvii. — minus distet a centro gravitatis sectoris, quolibet intervallo dato. Id. Prop. xlii. — sit quacunque linea recta data minor.*

115. "*Huygens de Quadratura Hyperbolæ, Ellipsis et Circuli, ex dato portionum gravit. centro, Theor. i. — quæ portionem excedat spatio, quod minus sit quovis dato. Id. Theor. ii — sit minor spatio quovis dato.*

116. "*James Gregory in Vera Circul. et Hyperbol. Quadratur. in Schol. Prop. vi. Continuando subduplam polygonorum descriptionem inveniri possunt duo polygona complicata, quorum differentia sit minor quamlibet exhibita quantitate.*

117. "*Simson Sect. Conic. Lib. v. Prop. xlviii. Portioni parabolæ, vel portionis dimidio circumscribi potest figura, aliaque in ipsa inscribi ex parallelogrammis æqualem latitudinem habentibus, quarum quæ circumscribitur portionem excedat, quæ vero inscribitur ab eadem deficiat spatio, quod minus sit spatio quovis dato.*

118. "And in all these propositions they always supposed a quantity first named, and then shew, how to make the approach expressed in the proposition. I have set down this large number of quotations, because the diversity of phrase, where-
" by

“ by these authors express the same sense, renders it
 “ impossible to be mistook. Though, I confess,
 “ this long disquisition in so evident a matter is much
 “ more than necessary; for had any of these writers
 “ been familiar to *Philaëthes*, he could not have had
 “ the least doubt of Sir *Isaac Newton*’s meaning.
 “ He might indeed have discovered it even from
 “ Sir *Isaac Newton* himself, who in the eleventh
 “ lemma has thrice interpreted the phrase *pro data*
 “ *quavis differentia* by these words, *pro differentia*
 “ *quavis assignata*: so that by *data* he could not
 “ mean assignable, unless *assignata* and *assignabilis*
 “ were synonymous words.

119. “ Again had *Philaëthes* been at all ac-
 “ quainted with the writers of indivisibles, he could
 “ not have attempted at that vain distinction be-
 “ tween the sense, in which these writers use the
 “ phrase infinitely small, and the sense, he imagines
 “ Sir *Isaac Newton* had assigned to it; that by
 “ infinitely small quantities, they meant a quan-
 “ tity fixed, determinate, invariable, but Sir *Isaac*
 “ *Newton* meant thereby a variable quantity; where-
 “ as it appears, that several writers in indivisibles
 “ on set purpose avoided expressions, that im-
 “ plied any thing fixed, determinate, or invariable.
 “ Monsieur *Pascal*, though he never imagined,
 “ that he did not follow the method of indivisibles,
 “ on the contrary he particularly at page 10 of his
 “ *Letters* published under the name of *Dettonville*,
 “ defends himself for so doing; yet is so cautious,
 “ as to avoid the word infinite in expressing their
 “ number, but constantly calls it indefinite; and
 “ certainly, when their number is considered as inde-
 “ finite, it cannot be pretended, that their magnitude
 “ was supposed fixed, determinate, and invariable.

120. “ Again Dr. *Barrow*, who has declared
 “ himself very expressly in favour of indivisibles,
 “ and defends his using of them, almost constantly

“ applies the term indefinite both to the number,
 “ and the magnitude of these particles. In his first
 “ Lecture on *Archimedes* he says, *Ponatur circulum*
 “ *esse figuram regularem habentem latera indefinite*
 “ *multa et parva, &c.* In his second Lecture,
 “ Prob. ii. *Supponatur cylindrum esse prisma quod-*
 “ *dam super polygonam basem, latera habentem in-*
 “ *definite parva et multa; and Prob. ix. Posito igitur*
 “ *VM esse infinite (vel indefinite) parvum.* Again
 “ in his fifth geometrical Lecture, Art. 6. *arcus*
 “ *MN indefinite parvus ponatur.* In his Differential
 “ Method of Tangents (as Sir *Isaac Newton* calls
 “ it in the Philosophical Transactions, N^o 342,
 “ pag. 197; or Comm. Epistolic. pag. 29.) at the
 “ end of his tenth Lecture, he says, *curvæ arcum*
 “ *MN indefinite parvum statuo.* In Lecture xi.
 “ Art. 1. *æquisecetur recta VD indefinite punctis A,*
 “ *B, C; and a little after ob indefinitam sectionem cur-*
 “ *vula GH pro recta haberi potest.* In Lecture xii.
 “ at the beginning, *arcum MN indefinite parvum*
 “ *esse; and at Art. 1. Spatium vero $\alpha\beta\delta$ minime*
 “ *differt ab indefinite multis reſtangulis, qualia $\mu\delta$;*
 “ *&c.* Again in his ninth Optical Lecture, Art. 12.
 “ *arcus NR, PS ex hypothesi sunt indefinite parvi*
 “ *(ſeu minimi).* Lect. 13. Art. 24. *ob ſumptam arcuum*
 “ *indefinitam parvitatem.*

121. “ Thus it appears, that Dr. *Barrow* was ſo
 “ far from conſidering theſe infinitely ſmall quan-
 “ tities as fixed, determinate, invariable, that he
 “ has purpoſely choſen the moſt looſe and indeter-
 “ minate expreſſions, he could contrive, to denomi-
 “ nate them by. If *Philalethes* had been in the leaſt
 “ acquainted with the writings of this moſt excel-
 “ lent geometer, ſurely it would have been impoſſi-
 “ ble for him to have aſſerted, that in all Sir *Isaac*
 “ *Newton*’s works there is no paſſage, no expreſſion
 “ to be found, that ſhould reaſonably make him ſo
 “ much as ſuſpected of uſing indiviſibles. Has he
 “ not

“ not throughout his *Lectiones Opticae* upon all occasions used either the same, or even less guarded expressions than these of Dr. Barrow ?

122. “ Sir Isaac Newton’s doctrine of prime and ultimate ratios is not to be defended from the accusation of resembling indivisibles by any minute variations, that may be found in his expressions, from what are used by the writers, who followed that other doctrine; for those writers are by no means exactly uniform with one another, but some have expressed themselves with more and some with less caution. What separates the doctrine of prime and ultimate ratios from indivisibles is the declaration made in the *Scholium* to the first Section of the *Principia*, that Sir Isaac Newton understood by the ultimate sums and ratios of magnitudes no more than the limits of varying magnitudes and ratios, and he puts the defence of his method upon this, that the determining any of these limits is the subject of a problem truly geometrical. To insist, that the variable magnitudes and ratios do actually attain, and exist under these limits, is the very essence of indivisibles. For in supposing this, we pretend to see directly, as Mr. Robins has expressed it, in these last forms or limits the properties, which the variable figures had before; and under this notion these limits must be allowed capable of being compared together by a direct form of demonstration. The impossibility only of this actual coincidence obliged Sir Isaac Newton to build his demonstrations concerning these limits upon the negative form by *deductio ad absurdum*.

123. “ If a choice of expression only were sufficient to distinguish between the two methods, *Hewariet* and Dr. Wallis may both be supposed to have avoided indivisibles, though they make no such pretensions. I here allude to this passage

“ of Heuraet at the end of the first vol. of *Carte's*
 “ *Geometry*. Unde cum illud [*nempe quod supra de-*
 “ *monstraverat*] verum sit, quotcunque reſtanguſa atque
 “ *tangentes extiterint*; et figura ex parallelogrammis
 “ *conſtans*, ſe eorum numerus in infinitum augeatur deſti-
 “ *nat in ſuperficiem AGHIKLF*, ac *tangentes ſimiliter*
 “ *in lineam curvam ABCDE*, liquet *ſuperficiem*
 “ *AGHIKLF æqualem eſſe reſtanguſo ſub Σ et reſta*
 “ *æquali curvæ ABCDE*. Again Dr. *Wallis* in his
 “ *treaſiſe of the Cycloide, &c.* (*Oper. Tom. i.*
 “ *p. 563.*) expreſſes himſelf thus,—*reſtas oi, oc*
 “ *angulo contactus ſubtenſas pro diminutione oc, oi,*
 “ *tangentium ita minui, ut illæ ad has rationem tandem*
 “ *ſubeant data quavis minorem*; ideoque evaneſcentibus
 “ *oi, oc tangenti et curvæ interjeſtis, coincident tum*
 “ *oc, tum oi, tangentiſ particulæ, particulis cur-*
 “ *væ oo.*

124. “ Again *Philaletheſ* is but imperfectly in-
 “ ſtructed in the precepts of common algebra; eſe
 “ he could not have imagined an incurvated line,
 “ which, he ſaw, would meet a right line in an
 “ infinite number of points, to be one and the ſame
 “ curve, and expreſſible by a finite equation;
 “ whereas the number of interſections of a right line
 “ with every algebraick curve is limited, the num-
 “ ber of ſuch interſections determining the order of
 “ the curve*.

125. “ Farther, it was only owing to his little
 “ exerciſe in geometrical ſubjects, that made him
 “ unapprized of Mr. *Robins's* meaning, when he
 “ ſpoke in general of vaniſhing quantities, as
 “ if they might be capable ſometimes of bear-
 “ ing the ratio, which he calls their ultimate;
 “ for had *Phi'aletheſ* been as well verſed in the
 “ writings of Sir *Iſaac Newton*, as might have
 “ been expected in one, who has appeared in his

* *Difſertation*, § 54, in the note

“ *deſenſe*,

“ defence, he never could have imagined, that be-
 “ cause Mr. *Robins* admits, that some quantities
 “ capable of an actual equality might be brought
 “ under the *lemma* so often mentioned, therefore he
 “ began to think himself obliged to allow an actual
 “ equality, where *Philaethes* contends for it. But
 “ above all it is most astonishing, that *Philaethes*
 “ should have taken so little care to understand the
 “ person he is writing against, as must be supposed,
 “ if we are to think him indeed sincere in his accu-
 “ sation, that Mr. *Robins* has given no less than four
 “ different interpretations of this *lemma*.

126. “ With these specimens of *Philaethes*’s im-
 “ perfect knowledge in the mathematicks, it would
 “ have been more becoming him to have been some-
 “ thing less free of his censures, and not so hastily
 “ to have charged with gross errors, false reasoning,
 “ and self contradiction a person, who at least seems
 “ to have used better endeavours to be well instructed
 “ than himself; nor should he be at all surpris’d, if
 “ he has miss’d in any measure Sir *Isaac Newton*’s
 “ meaning, who has wrote in a stile, which sup-
 “ poses his reader thoroughly conversant in geome-
 “ trical subjects.”

127. Thus Mr. *Robins* concluded his Remarks on
 the Considerations, which *Philaethes Cantabrigiensis*
 thought fit to publish on the foregoing Dissertation.

A
D E M O N S T R A T I O N
O F

The Eleventh Proposition of Sir Isaac Newton's Treatise of Quadratures. First printed Anno 1727, in the Philosophical Transactions, N^o 397; and afterwards somewhat altered in the Abridgment made by Mess. Reid and Gray, Anno 1733.

THIS Proposition consists of two parts: the first is as follows.

Let there be any curve ADI, whose abscisse AB shall be denoted by z , and its ordinate BD by y ; which may be related in any manner to the abscisse. And calling this the first curve, let other curves AEK, AFL, AGM, AHN, &c. be formed to the common abscisse AB or z , by making the ordinate BE of the second curve always equal to the area ABD of the first divided by unity; the ordinate BF of the third equal to the area ABE of the second divided by unity; the ordinate BG of the fourth equal to the area ABF of the third divided by unity; and so on continually.

Suppose now, that other curves AOS, APT, AQV, ARW, be described to the same common abscisse AB or z ; in which curves the ordinate BO of the curve AOS shall be equal to zy , the ordinate
BP

BP of the curve APT equal to z^2y , the ordinate
BQ of the curve AQV equal to z^3y , the ordinate
BR of the curve ARW equal to z^4y , &c. And let
the whole area ACI be denoted by A, the area ACS
by B, the area ACT by C, the area ACV by D,
the area ACW by E, &c. Then the series of
curves ADI, AEK, AFL, AGM, AHN, are thus
measured.

The area of the first curve ADI is = A

— of the second AEK is = $zA - B$

— of the third AFL = $\frac{zzA - 2zB + C}{2}$

— of the fourth AGM = $\frac{z^3A - 3z^2B + 3zC + D}{6}$

— of the fifth AHN = $\frac{z^4A - 4z^3B + 6z^2C - 4zD + E}{24}$

and so on perpetually.

Here in all the curves following the first, the index of the highest power of z is always the number, which expresses the distance of the curve from the first, and afterwards decreases regularly by unity; the first term is multiplied into A, the second into B, the third into C, the fourth into D, and so on; the coefficients are the same as in a binomial raised to the highest power of z , and the divisor is so many terms of this progression $1 \times 2 \times 3 \times 4 \times 5 \times 6$ &c. as is expressed by a number equal to the highest index of z .

Otherwise supposing n to represent the distance of the curve to be measured from the first; then the area sought will be found by extending $\overline{z-1}^n$ into a series, and multiplying the first term by A, the second by B, the third by C, the fourth by D, &c. and dividing the whole by $n \times n - 1 \times n - 2$, &c. continued to unity.

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SECOND PART.

Supposing the first, second, third, &c. curves to be the same as before. Let t denote the whole abscisse AC, and put x for BC. Then describe the curves CXA, CYA, CZA, CFA, where BX shall be equal to xy , $BY = x^2y$, $BZ = x^3y$, $BF = x^4y$, &c. This being done, and in the series of curves CIDA, CXA, CYA, CZA, CFA, &c. the first area CIDA being put equal to P, the second CXA equal to Q, the third CYA = R, the fourth CZA = S, the fifth CFA = T, &c. the whole areas of the aforesaid series of curves are also determined, as follows.

$$\text{The first AIC} = P$$

$$\text{The second AKC} = Q$$

$$\text{The third ALC} = \frac{1}{2}R$$

$$\text{The fourth AMC} = \frac{1}{6}S$$

$$\text{The fifth ANC} = \frac{1}{24}T.$$

Here the areas P, Q, R, S, T are divided by numbers produced by multiplying as many terms of this series $1 \times 2 \times 3 \times 4 \times 5$ &c. together, as in the former case.

Demonstration of the First Part.

Since each of the curves AEK, AFL, &c. are formed on the common abscisse z , by every where taking for ordinates the area of the preceding curve, we are to demonstrate, that in the expressions A, $zA - B$, &c. each following expression is the area of the curve, that has for its ordinate that expression, which preceded it, and z for its abscisse.

Suppose $m = n - 1$;

$$z^m A - \frac{m}{1} z^{m-1} B + \frac{m}{1} \times \frac{m-1}{2} z^{m-2} C \text{ \&c.}$$

$$\text{Then } \frac{m \times m-1 \times m-2 \text{ \&c.}}{1 \times 2 \times 3 \times 4 \times 5 \text{ \&c.}}$$

$$z^n A - n z^{n-1} B + \frac{n}{1} \times \frac{n-1}{2} z^{n-2} C \text{ \&c.}$$

$$\text{And } \frac{n \times n-1 \times n-2 \text{ \&c.}}{1 \times 2 \times 3 \times 4 \times 5 \text{ \&c.}}$$

are

are by the proposition two expressions following each other, we are to shew, that the second of them is equal to the area of a curve, which has the first for its ordinate, and z for its abscisse. The fluxion of this last mentioned area is $\dot{z}$ into the first of these expressions; we are to shew, that the second of these expressions is the fluent of that fluxion, or which is the same, that the fluxion of this second expression is equal to $\dot{z}$ into the first expression.

The fluxion of the second is

$$nz^{n-1}\dot{z}A - \overline{n-1} \times \frac{n}{1} z^{n-2}\dot{z}B + \overline{n-2} \times \frac{n}{1} \times \frac{n-1}{2} z^{n-3}\dot{z}C \mathcal{E}c. \\ + z^n \dot{A} - \frac{n}{1} z^{n-1} \dot{B} + \frac{n}{1} \times \frac{n-1}{2} z^{n-2} \dot{C} \mathcal{E}c.$$

the whole divided by $n \times n - 1 \times n - 2 \mathcal{E}c.$ Here the first line consists of the fluxions of the powers of z into $A, B, C, \mathcal{E}c.$ and the second line is the fluxions of $A, B, C, \mathcal{E}c.$ into the powers of z . But each of the terms $z^n \dot{A}, z^{n-1} \dot{B}, z^{n-2} \dot{C} \mathcal{E}c.$ in the second line are equal to $z^n \dot{z}y$, since by the formation of the curves that $A, B, C, \mathcal{E}c.$ are the areas of, $\dot{A} = \dot{z}y, \dot{B} = \dot{z}zy, \dot{C} = \dot{z}zzy$. The second line of this fluxion

is therefore equal to $1 - \frac{n}{1} + \frac{n}{1} \times \frac{n-1}{2} \mathcal{E}c.$ $\times z^n \dot{z}y$, that is, equal to $1 - n^a \times z^n \dot{z}y = 0$, the whole fluxion by this means becomes

$$\frac{nz^{n-1}\dot{z}A - \overline{n-1} \times \frac{n}{1} z^{n-2}\dot{z}B + \overline{n-2} \times \frac{n}{1} \times \frac{n-1}{2} z^{n-3}\dot{z}C \mathcal{E}c.}{n \times n - 1 \times n - 2 \mathcal{E}c.}$$

Then striking out the common multiple n from the top and bottom, and dividing by $\dot{z}$, we have the same quantity equal to

$$\frac{\dot{z} \times z^{n-1} A - \frac{n-1}{1} z^{n-2} B + \frac{n-1}{1} \times \frac{n-2}{2} z^{n-3} C \mathcal{E}c.}{n-1 \times n-2 \times n-3 \mathcal{E}c.}$$

and substituting $m, m-1, m-2, \mathcal{E}c.$ for $n, n-1, n-2, n-3, \mathcal{E}c.$ the same becomes

$\dot{z} \times$

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$$\frac{\dot{z} \times z^m A - \frac{m}{1} z^{m-1} B + \frac{m}{1} \times \frac{m-1}{2} z^{m-2} C \mathcal{E}c.}{m \times m - 1 \times m - 2 \mathcal{E}c.}$$

This then, which is the fluxion of the second expression, is shewn to be equal to $\dot{z}$ into the first expression; consequently each following term is the area of a curve, that has the preceding term for its ordinate, and z for its abscisse.

Demonstration of the Second Part.

Suppose any curve whose distance from the first is denoted by n ; we are to prove, that the curve, whose abscisse is BC or x , and its ordinate $x^n y$ divided by $n \times n - 1 \times n - 2 \times n - 3 \mathcal{E}c.$ continued to unity, will be equal to it, when x is equal to AC or t .

It is evident, that when the areas ABD , ABO , ABP , ABQ , ABR , $\mathcal{E}c.$ decrease, the areas $BCID$, $BCSO$, $BCTP$, $BCVQ$, $BCWR$ increase respectively; and consequently the decrements of the areas ABD , ABO , ABP , $\mathcal{E}c.$ or their fluxions with a negative sign, are the increments or fluxions of the areas $BCID$, $BCSO$, $BCTP$, $\mathcal{E}c.$ that is, calling the areas ABD , a ; ABO , b ; ABP , c ; ABQ , d ; and $BCID$, α ; $BCSO$, β ; $BCTP$, γ ; $BCVQ$, δ ; $BCWR$, ϵ ; then $\dot{\alpha} = -\dot{a}$, $\dot{\beta} = -\dot{b}$, $\dot{\gamma} = -\dot{c}$, $\dot{\delta} = -\dot{d}$, $\dot{\epsilon} = -\dot{e}$.

Now the fluxion of the curve, whose abscisse is $= x$ or BC , and its ordinate $= x^n y$, is $\dot{x} x^n y$; that is, equal to $\dot{x} y \times t - z^n$; n being $= t - z$; or since the increment of x , or $\dot{x}$ is equal to the decrement of z , or $-\dot{z}$, the fluxion of the same curve is equal to $-\dot{z} y \times t - z^n = -\dot{z} y$ in $t^n - n \times t^{n-1} z + n \times \frac{n-1}{2} t^{n-2} \times z^2 \mathcal{E}c. = -t^n \dot{z} y + n t^{n-1} \dot{z} z y - n \times \frac{n-1}{2} t^{n-2} \dot{z} z^2 y, \mathcal{E}c.$ that is $= t^n \times$

$$- \dot{a} - nt^{n-1} \dot{x} - \dot{b} + n \times \frac{n-1}{2} t^{n-2} \dot{x} - \dot{c}, \text{ \&C.}$$

$$\text{or} = t^n \dot{a} - nt^{n-1} \dot{\beta} + n \times \frac{n-1}{2} t^{n-2} \dot{\gamma}, \text{ \&C. and}$$

taking the fluents, the area of the curve, whose abscisse is x , or BC, and ordinate $x^n y$, is equal to

$$t^n \alpha - nt^{n-1} \beta + n \times \frac{n-1}{2} t^{n-2} \gamma, \text{ \&C. But when}$$

x is equal to AC, then $\alpha, \beta, \gamma, \text{ \&C.}$ will be equal to A, B, C, \&C. as is very evident; consequently the area of the curve, whose abscisse is x and ordinate $x^n y$, when x is equal to AC, is $t^n A - nt^{n-1} B$

$$+ n \times \frac{n-1}{2} \times t^{n-2} C, \text{ \&C. that is, equal to } \overline{t-1}^n$$

thrown into a series, and the first term multiplied by A, the second by B, the third by C, \&C. But

$\overline{t-1}^n$ thrown into a series, and the first term multiplied by A, the second by B, the third by C,

\&C. and then the whole divided by $n \times n - 1 \times n - 2$, \&C. continued to unity, is equal to the area

of the curve, whose place in the series is denoted by n . Therefore the area of the curve, whose abscisse is equal to x , and its ordinate to $x^n y$, taken

when x is equal to AC, and divided by $n \times n - 1 \times n - 2 \times n - 3$, \&C. continued to unity, is e-

qual to the area of a curve, whose place in the series is denoted by n ; that is, Q, which is the area of

a curve, whose abscisse is x , and ordinate xy taken when x is equal to AC, is equal to the second curve

AKC; half R, which is the area to the abscisse x and ordinate $x^2 y$, taken in the same manner, is e-

qual to the third curve ALC; $\frac{1}{6}$ S, which is a like area to x and $x^3 y$, is equal to the fourth curve

AMC; $\frac{1}{24}$ T, the area to x and $x^4 y$, x being equal to AC, is equal to the fifth curve ANC; and so

on perpetually. Q. E. D.

REMARKS

R E M A R K S

O N

*A Treatise lately printed at Paris, and entitled,
Discours sur les Loix de la communication du
Mouvement, par Monf. Bernoulli.*

*First published in The Present State of the Republick
of Letters for May 1728.*

THE Royal Academy of Sciences at *Paris* having proposed, in the years 1724 and 1726, two questions, one relating to the laws of motion and percussion, the other concerning the cause of elasticity; Mr. *John Bernoulli*, professor of the mathematicks at *Basle*, sent them some papers of his, wherein he endeavoured to prove the opinion of Mr. *Leibnitz*, in relation to the laws of motion. These papers have since been published at *Paris*, and I intend to make some remarks on them.

Mr. *Bernoulli*, chap. i. § 5. in order to prove that there are no bodies perfectly hard or inflexible, lays it down as an immutable law of nature, that no body can pass from motion to rest instantaneously, or without having its velocity gradually diminished.
That

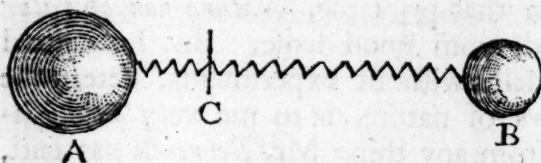
That this is a law of nature, Mr. *Bernoulli* thinks is evident from that principle, *Natura non operatur per saltum*, and from good sense. But how good sense can of itself, without experiments, determine any of the laws of nature, is to me very astonishing. Indeed from any thing Mr. *Bernoulli* has said, it would have been altogether as conclusive to have begun at the other end, and have disputed, that no body can pass instantaneously from motion to rest; because it is an immutable law of nature, that all bodies shall be flexible.

In §. 18, Mr. *Bernoulli* defines perfect elasticity to be a power in a body, by which it restores itself to the same figure after compression, that it had before, independent of the readiness with which it restores itself; and a spring or body perfectly elastic in this sense will, he assures us (chap. ii. §. 2), produce the same velocity in a contiguous body by restoring itself, that it destroyed in being compressed. But this is contrary to experience, since there are great numbers of bodies perfectly elastic according to this definition, or such as perfectly restore themselves; but none are known, that will communicate the same velocity by their restitution, that they destroyed by their compression.

Mr. *Bernoulli* affirms chap. ii. §. 3, that a spring must communicate the same velocity to a contiguous body in restoring itself, which it destroyed when compressed; for in this, says he, consists the equality of action and re-action. But philosophers understand no more by the equality of action and re-action, than that when one body acts on another, that other re-acts as much on the first at the same instant; yet Mr. *Bernoulli* here affirms, that the spring, when it restores itself, will act on the body as much, as the body acted on the spring before, while the spring was bending; which is a proposition altogether different from the former.

In

In chap. iii. §. 4. Mr. *Bernoulli* observes, that if



the spring AB bent between the two bodies A and B is divided in C in

a reciprocal proportion to those bodies, then the point C will rest, while the whole spring by expanding itself communicates velocities to those bodies: whence he would conclude, that the springs between A and C only act on the body A, and those between C and B on the body B, whereas all the springs must certainly act against each body; for the part CB could not act at all against the body B, if the other part CA did not support the point C; and indeed of this his sixth chapter, if rightly considered, will be found to contain an ample proof.

And here I would observe, that the forces communicated to these two bodies are communicated by an equal pressure acting an equal time on each, as Mr. *Bernoulli* allows. Now how these two bodies can receive by this means different forces, much less forces, that may be in any proportion assignable, is to me very wonderful; and yet is what really will happen, if Mr. *Bernoulli's* hypothesis is true, since those bodies receive in this case velocities reciprocally as their masses, as he himself allows.

Mr. *Bernoulli* in his fifth chapter, takes a great deal of pains to shew the difference between what, he calls force *morte*, and force *vive*; or, as we shall translate it, the *inactive* and *vivid* force of bodies; that is, between pressure, and the force of a body in motion; and amongst the rest of his distinctions, he says §. 6, that the acting of any spring or pressure upon a body supported by an immoveable obstacle does not at all diminish the force of that spring or pressure, as the force of air, condensed in a recipient is not at all diminished by the force, it exerts

against

against the sides of the recipient ; but that no velocity or vivid force can be communicated to any body without the loss of so much force in the pressure, that produced it ; as the elasticity of the condensed air would be diminished, if it acted against any body placed in the mouth of the receiver, and communicated any degree of velocity to it.

But here is a mistake, since the elasticity is not diminished, because the air has spent any of its force by acting against the body, and by communicating a degree of velocity to it ; but because by that means it comes to possess a space greater than before, whereby its expansive force decreases. Now if any fluid had the same elastic force in different degrees of density, so as not to be weakened by expanding, such a fluid would not lose any of its force by putting a body in motion.

Besides Mr. *Bernoulli* here contradicts himself in asserting, that the production of any degree of vivid force requires the loss of an equal degree of force in the spring or pressure, that produces it ; when he has been arguing throughout the preceding part of this chapter, that the force of a spring or pressure is entirely different in nature from what, he calls vivid force ; and therefore to speak of the force of a spring or pressure, as equal to any degree of vivid force, is on his own principles an absurdity ; and equally absurd is the seventh and eighth sections of this chapter, and the ninth section of the tenth chapter.

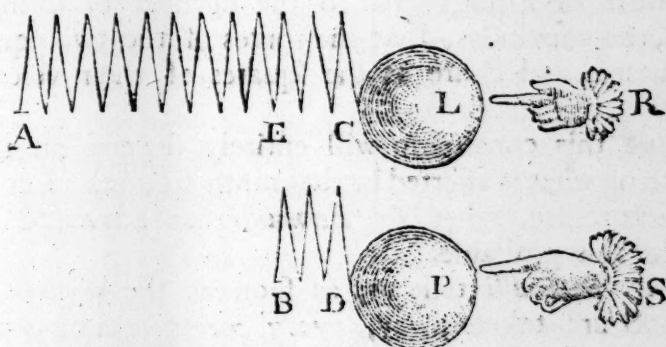
Mr. *Bernoulli*, in chap. v. §. 2, tells us, that Mr. *Leibnitz* was the first, who discovered the force of a body in motion to be as the square of the velocity, it is moved with.

Mr. *Leibnitz* was indeed the first, who mentioned it, but in such a manner as was little to his honour, giving it by mistake for the common opinion ; for in a tract in the *Acta Eruditorum*, A. D. 1686, where

he endeavours to prove the error of *Cartes* in asserting the same quantity of motion to be always preserved in the world, he says, that it is agreed on by the *Cartesians*, and all other philosophers and mathematicians, that there is the same force requisite to raise a body of one pound to the height of four yards, as to raise a body of four pounds to the height of one yard; but being shewn, how widely he was mistaken in taking that for the common opinion, which would, if allowed, prove the force of the body to be as the square of the velocity it moved with, he afterwards, rather than own himself capable of such a mistake, endeavoured to defend it as true; since he found it was the necessary consequence of what, he had once asserted; and maintained, that the force of a body in motion was proportional to the height from which, it must fall to acquire that velocity. Which assertion alone contains, in effect, all the arguments, that *Leibnitz* and his followers have hitherto chiefly relied on.

Mr. *Bernoulli* in §. 13 observes, that when Mr. *Leibnitz*'s adversaries contended for taking into consideration the time of the body's rising or falling, they were not aware, that a body may be made to rise to different heights in the same time by means of an inverted cycloid. But here Mr. *Bernoulli* himself is overseen; and does not consider, that bodies, which rise to different heights in a cycloid in the same space of time, are not stopped by the same force; for the power of gravity acts upon these bodies but with a part of its whole force, and the action upon each is proportional to the space through which, it moves. This therefore is as great a paralogism, as it would have been to have computed the forces of any body moving with different velocities from the heights, it fell to acquire these velocities, though sometimes it fell in air, and in other cases in quicksilver.

In order to consider the sixth and seventh chapters, it will be necessary to explain a scheme, that he constantly refers to. AC is a range of springs, one ex-



tremitry of which is fixed at the point A; each of these springs is kept bent to a certain angle by the power R, which presses the body L against the other extremity C. BD is another range containing a less number of springs, which are fixed at B, and bent to the same angles by the power S, which presses the body P against D.

Now Mr. *Bernoulli* says, that supposing the springs AC equal in strength to the springs BD, when bent to the same angle; or, which is the same thing, that the powers R and S, and the bodies L and P are equal; then the velocity communicated to the body L by the springs AC in expanding themselves to any larger angles (the power R being taken away) will be to the velocity communicated to the body P by the springs BD in expanding themselves to the same angles (the power S being taken away) in the subduplicate proportion of the number of springs AC to the number of springs BD.

This he demonstrates by shewing the time, at which AC acts against L in any given inflexion, to be in that proportion to the time, that BD acts against P in the same inflexion, when their forces are equal. See §. 1. and 2. chap. vii.

And he having asserted, §. 9. chap. vi. that the force communicated to L in this case, is to the force communicated to P in the simple proportion of the number of springs AC to the number of springs BD, he concludes, that the forces of the two equal bodies L and P are as the squares of their velocities.

But this conclusion will entirely depend on the truth of what is asserted in that ninth section. Let us therefore see, what Mr. *Bernoulli* has advanced to make this probable.

Mr. *Bernoulli* tells us, as soon as the powers R and S are taken away, every spring exerting its force, and none of it perishing unusefully, the force of every one of these springs must of necessity be employed to produce its effect; which effect can be nothing else but moving the bodies. Therefore he says, the motion of each body will be such, that its vivid force shall be precisely equal to the total effect of that, which the springs taken together shall have contributed to it; but as each of these springs dilates itself equally from one given angle to another, every one of them contributes equally to produce that force; therefore the vivid forces produced in the bodies L and P; will be as the number of springs, that have contributed to their production.

I shall pass by the equality between pressure and vivid force, which Mr. *Bernoulli* seems to insinuate in this section, (see remark on chap. v.) and shall only take notice, that Mr. *Bernoulli* asserts, that each of the springs in each range contributes equally to the production of the force in the impelled bodies.

But how is this to be proved? Mr. *Bernoulli* has taken a great deal of pains in this same chapter to shew, that the pressure in each range is equal, when all the springs in both are bent to the same angle;
how

how then does it appear, that the forces produced in these bodies will be anywise different, since produced by equal pressures? Mr. *Bernoulli* endeavours to solve this by saying in his sixth section, that if the point E in the longer range was fixed, so that the number of springs included between E and C be the same with those between B and D; then the springs between E and C would produce the same acceleration in the body L, as the springs BD equal in number would produce in an equal body P; and therefore the point E not being fixed, the remaining springs between A and E by dilating themselves will act on the point E, and produce a greater acceleration in the body L than the springs EC alone, or than the springs BD produce in P.

But this is overthrowing all the principles, that he has been establishing in the foregoing part of this chapter; for if it means any thing, it must mean, that the springs AE and EC together will act with greater force on the body L, than the springs EC alone, contrary to the whole tenor of the second and third sections.

If Mr. *Bernoulli* shall at any time give the true reason, why the springs AC and BD will produce different forces in the bodies L and P, which without doubt is the different times of their acting on those bodies, this will at once destroy all his hypothesis; for then the force of the body L will not be to the force of the body P, in the proportion of the number of springs AC to the number of springs BD, or in the duplicate proportion of their velocities, but in the direct proportion of the velocities, or the subduplicate of the number of springs, seeing the forces estimated from the pressures, and times of their acting conjointly, must be in the same proportion with the velocities, because Mr. *Bernoulli* himself in §. 1. and 2. chap. vii. has estimated the velocities from the very same principles.

Mr. *Bernoulli*, chap. viii. §. 2. says, that this opinion has been confirmed by experiments made of the cavities formed in soft substances, such as clay, &c. by bodies falling with different velocities; it being always found, that the cavities formed by the same body were as the squares of the velocity it fell with; whence it has been concluded, that the force of the same body is as the square of the velocity, it moves with.

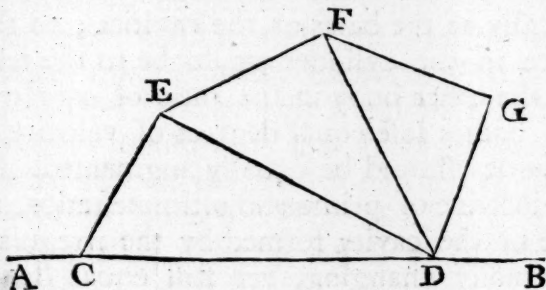
But all, that can be inferred from these experiments, is, that the resistance of such substances is equable or proportional to the time only, that they act against an immersing body; for, if the body, that forms the cavity, is a cylinder, which moves in the direction of its axis, then, since *Galileo* has demonstrated, that the height any body will rise to, when retarded by an equable resistance, like gravity, will be as the square of its velocity, it is evident, that the depth, such a cylinder will sink in a substance resisting equably, will be in the same proportion, and the cavity will be as that depth, since the cylinder moves in the direction of its axis. Moreover, the cavity formed by any other body, however irregular, will be equal to the cavity formed by a cylinder of the same weight moving with the same velocity; for whenever the cylinder, and other body should begin to form equal augmentations of their respective cavities with equal degrees of velocity, the resistance of the cylinder to the resistance of the other body, would be compounded of the proportion of the base of the cylinder to the surface directly opposed to the motion of the other body, and the proportion of the time of the cylinder's forming this augmentation in its cavity to the time, in which an equal augmentation would be made by the other body, provided the base of the cavity of that other body remained the same during the time of this augmentation, and these times would be as the

the depths of the augmentations of cavity, that is, reciprocally as the bases of the cavities; so that the resistance in the cylinder would be to the resistance against the other body in the ratio of equality, and the two bodies lose equal degrees of velocity, while their cavities should be equally augmented. Hence, by the doctrine of prime and ultimate ratios, though the base of the cavity formed by the irregular body is perpetually changing, yet still equal degrees of velocity will be lost during equal augmentations of the cavities; therefore if they enter the yielding substance with equal degrees of velocity, they will both, in losing their entire velocities, form equal cavities.

What Mr. *Bernoulli* has said, §. 4. is only a consequence of what, *Huygens* has demonstrated from the other opinion, and shall be considered in another place. See remarks on chap. x. §. 1.

Mr. *Bernoulli* intitles his ninth chapter, A general and geometrical demonstration, that the quantity of the force of a body is proportional to its mass, drawn into the square of its velocity; which, he says (without insisting longer on the validity of the precedent argument) is so general, and so very much beyond all exception, that he believes it capable of convincing the most obstinate adherers to the vulgar opinion.

This demonstration, when stripped of what is not essential to it, is thus. Suppose the motion of any body moving in the direction AB to be represented by CD, and suppose the triangles CED, EFD and FGD to be right-angled at the points E, F and G, and the sides CE, EF, FG and GD, equal to each other; then, as the sum of all their squares is equal to the square of CD, each of those lines will be half CD; now, by the composition of motion, the motion of the body in the direction AB with the velocity proportional to CD, is equivalent to the mo-



tion of the same body in the directions, and with velocities proportional to CE, ED, or to the motion in the directions CE, EF, FD, or in the directions CE, EF, FG, GD, with velocities, proportional to these respective lines; but each of those lines is half the line CD, therefore the motion in CD with any velocity, is equivalent to the motion in CE, EF, FG, GD, with half the velocity in CD; hence Mr. *Bernoulli* concludes, that the force of a body moving with any velocity, is equal to four times the force of the same body moving with half that velocity, or that the force of the same body is as the square of the velocity, it moves with.

But this proposition, if it proves any thing, proves, that the forces of the same body moving with different velocities, are not only as the squares of the velocities, but in any assignable proportion to them; for as this proposition will hold good, whether the angles CED, EFD, &c. are right or not, you may by varying those angles find any number of equal lines CE, EF, FG, &c. the motions in all which shall compound the motion in CD, a line of any given length.

Mr. *Bernoulli* in his tenth chapter, §. 1. says, that the sum of the forces of any elastic bodies remains the same after the stroke, that it was before. This he endeavours to prove by saying, that the augmentation of the force of the one is always the immediate effect of the diminution of the force of the

the other ; but this is manifestly false. For, if two elastic bodies meet each other with contrary velocities, then on their first compression it is evident, that both their forces are diminished at the same time ; and it may happen, that in restoring themselves, both their forces may be increased at the same time. As this principle is false, his deductions from it must of course be so too ; and therefore his proving, that the sum of the squares of the velocities of any two elastic bodies, is (when multiplied into those bodies) the same before and after the stroke, is so far from being a confirmation of his method of estimating the force of bodies, that it is a contradiction to it, and not only so, but a direct consequence of the other opinion. For it is evident from the equality of action and re-action, that the loss or increase of the force of one body in one direction, is equal to the loss or increase of the force of the other body in a contrary direction : Now if you estimate the forces so lost, by the sum or difference of the velocities of the bodies before and after the stroke drawn into the bodies themselves, according to the common opinion, then you will find, that on these principles the sum of the squares of their velocities multiplied into their masses, will be the same both before and after the stroke, as *Huygens* has demonstrated.

From this equality of the sums of the squares, &c. before and after the stroke, Mr. *Bernoulli* also asserts, that the same degree of motion will always be preserved in the world ; but he did not consider, that this equality holds in bodies perfectly elastic only, and there are none such in nature ; therefore this hypothesis is false, allowing all his own principles. See chap. x. §. 5. chap. v. §. 9, 10, &c.

In the eleventh chapter Mr. *Bernoulli* has solved a particular case of the general problem relating to the stroke of three elastic bodies ; which is, when one body in motion strikes at the same time two other

other equal bodies at rest, which are equally distant from the line of the direction of its center on each side; and in his twelfth chapter he extends it to the striking of one body, at the same time, against any number of pairs of bodies at rest, each pair being supposed equal, and equally distant on each side from the line of its direction. Mr. Bernoulli's method is first to solve the problem for two pair, and so by degrees extend the solution to any number. In two pair he supposes the moving body to be divided into two parts, one part of which strikes one pair, and the other part strikes the other pair; now by computing the velocity of each of these parts after the stroke, and making an equation between them, he determines the proportion, that the body must be divided in, so that each part after the stroke shall move with the same velocity; which common velocity, he concludes, will be the same with that of the entire body. But this will be true, only when each part takes up the same time in communicating motion to the bodies it strikes against; for, if one part communicates the whole motion to the bodies, it strikes against, sooner than the other, then the two parts would be separated, if the body was actually divided, and consequently the velocity of the body, when entire, will be different from the common velocity of the parts.

Now all elastic bodies, that we have any knowledge of, will take up different times to separate in, according to the different velocities they separate with; since it is found by experience, that their resistance to compression is uniform. However, to give that for a general solution of a problem, which may be false in infinite circumstances, and can be true but in one, is a manifest paralogism; especially when those circumstances are neglected, as no ways necessary to the solution.

In §. 2. of the same chapter he says, that by means of this theory, the resistance of a body moving in a fluid composed of elastic particles may be easily determined, and also the quantity of its motion, that it will lose in moving through any given space; a new subject for a search, as useful as curious, and so much more worthy to be examined, as no body hitherto has undertaken it. But I am surprized, that Mr. *Bernoulli* should mention this as something yet to be done; seeing Sir *Isaac Newton* has so amply treated this and all other parts of the doctrine of the resistance of fluids in the second book of his *Principia*.

Mr. *Bernoulli* in his fourteenth chapter endeavours to give the same rule for finding the center of oscillation from his theory of forces, that *Huygens*, Dr. *Taylor*, and even himself, have given from the common opinion. In order to this, in §. 4. he supposes, that the force of any body will be always the same, when it has fallen from the same height, whether it falls freely, or whether any obstacle retards its fall. If this is true, then the same body may have the same force with different velocities; since by increasing the obstacle, that hinders its descent, you may diminish its velocity in any degree assignable, still preserving the same height.

His hypothesis of the cause of elasticity is so full of inconsistencies and absurdities, that I am deterred from making any remarks on it, for I should be obliged to take notice of almost every passage. I shall therefore only mention one thing, which, I think, may easily excuse an ampler confutation, and that is, that Mr. *Bernoulli* endeavours to account for elasticity by the motion of particles, that are themselves elastic.

And now, I think, I have proved, that nothing Mr. *Bernoulli* has urged in defence of Mr. *Leibnitz's* opinion, is any way conclusive; that many parts of
his

his discourse are contradictory ; and that all his determinations of the laws of motion are wrong, since they are by him applied to bodies, which only perfectly restore themselves ; whereas they are true in none but such, as restore themselves in the same time, they were compressed. See remark on chap. 1. §. 18.

And for a conclusion, I would desire Mr. *Bernoulli* to give us a true solution of the problem proposed in his twelfth chapter ; for it will very much surprize the world, that so eminent a geometer should so unaccountably mistake (as I have shewn in my remarks on that chapter) in the solution of a problem, the only important one in his book, that had not been solved before.



A N
E X A M I N A T I O N
O F T H E
N O T E

Concerning the Sun's parallax, published at the end of a book entitled Matho; five Cosmotheoria puerilis dialogus, in Quarto, Edinburgi 1738.

First printed in The History of the Works of the Learned for October 1738.

IN this Note the author endeavours to shew, that either the moon revolves about the earth in a manner altogether inconsistent with the general law of gravity, or that the quantity of matter in the sun and its parallax are very different, from what they are at present deemed to be.

For supposing the parallax of the sun to be about ten seconds (its usually assigned magnitude) he computes, that the gravitation of the moon to the sun would then be greater than her gravitation to the earth, by which she is supposed to be kept in her orbit; and thence he concludes, that, as in her conjunction she is placed between the earth and sun, her greater gravitation to the sun would at least in that

190 REMARKS ON MATHO.

that situation draw her out of her orbit, and carry her intirely away from the earth.

The gravitation of the moon to the sun is indeed greater than her gravitation to the earth; but the deductions of our author from this fact are altogether groundless.

There is no proposition in the whole theory of motion more incontestable and obvious, than that the relative motions of any system of bodies will be no ways affected by equal accelerating forces applied in parallel directions to each of the bodies, that compose it. Therefore, if to the earth and moon equal degrees of gravitation towards the sun were applied in parallel directions, no change in the relative motion of the moon about the earth would be thence produced; even though the gravitation to the sun were in each of them a thousand times greater than the moon's gravitation to the earth.

Consequently, from the absolute quantity of the moon's gravitation to the sun no inequalities will arise in her motion, unless that force be different, or differently directed from the force of the sun on the earth. But the diameter of the moon's orbit is so small, compared with the distance of the sun, that lines drawn from any points of that orbit to the sun, differ but little from being parallel to the line joining the earth and sun, and her gravitation to the sun in the several parts of her orbit is but little different from the gravitation of the earth to the sun.

It is by these differences only, and not by the whole gravitation of the moon to the sun, that the relative motion of the moon about the earth can be disturbed; and from them arise the inequalities in that motion observed by astronomers.

REMARKS

R E M A R K S

O N

Mr. E U L E R's Treatise of *Motion*,
Dr. SMITH's compleat System of *Opticks*,

A N D

Dr. J U R I N's Essay upon Distinct
and Indistinct *Vision*.

Hæc eo animo accipi velim, quo ego accipiam, quoties acciderit, ut aliquis mihi errores meos indicet.——Boni autem viri munus esse puto, non aliorum peccata dissimulare; sed potius omnes homines, si fieri posset, ab inscitæ tenebris in lucem veritatis asserere.

Petr. Nonius de Errat. Orontii Finæi. In præfat.

First published in 1739.

REMARKS

The first part of the book is devoted to a description of the various species of the genus *Amphibia*.

The second part of the book is devoted to a description of the various species of the genus *Reptilia*.

The third part of the book is devoted to a description of the various species of the genus *Avia*.

The fourth part of the book is devoted to a description of the various species of the genus *Mammalia*.

The fifth part of the book is devoted to a description of the various species of the genus *Pisces*.

The sixth part of the book is devoted to a description of the various species of the genus *Insecta*.

The seventh part of the book is devoted to a description of the various species of the genus *Plantae*.

The eighth part of the book is devoted to a description of the various species of the genus *Fungi*.

P R E F A C E.

1. **I**T has not been uncusomary amongst mathematicians, without any impeachment of their candour, by expresse treatises to correct important mistakes of their contemporaries. Thus the errors of Cardinal Cusanus were shewn by Regiomontanus, those of Oronius Finæus by Petrus Nonius, of Joseph Scaliger by Vieta and Clavius, of Longomontanus by Dr. Pell, of Gregory St. Vincent by Huygens, of Hobbes and Du Laurens by Dr. Wallis, and of Sinclair by James Gregory. And I shall here take leave after these examples to make some remarks upon two treatises, which have lately come abroad; Mr. Euler's discourse entitled *Mechanica sive motus Scientia analytice exposita*; and the Compleat System of Opticks by Dr. Smith.

2. FROM the time that Des Cartes advanced the calculations of algebra into the place of geometrical demonstrations, not only the works of the ancients, but at length the greatest part of mathematical writings have been neglected; and at present the mathematical sciences are generally considered as attainable by as little study as the operations of arithmetick. So much of the elements as can be learnt from some crude collection out of Euclide, with a few of the primary properties of the conic sections joined to some exercise in algebraical computations being thought at present, a stock sufficient to constitute a considerable geometer; and if to this be added a knowledge of the method of calculating used, where the doctrine of fluxions is required, without any true apprehension of the principles of that doctrine; the artist is supposed to have penetrated into the utmost depths of

this science. Though by this means not only all elegance and taste are lost, but many direct errors have been committed by persons, compleatly adorned with these modern accomplishments, in their writings on a subject, of which they had not read enough to render it familiar to them, or their conceptions concerning it clear and distinct. Whoever could in the least contribute to put a check to this inundation of barbarism, would certainly perform no inconsiderable service to a branch of learning, once esteemed the first of the sciences, but at present reduced to a kind of mechanical practice, wherein the reasoning faculties are little concerned.

3. IN the first of the treatises I design to examine, the author has unfortunately followed the principles of his calculus with so little caution, as even to contradict Euclide himself *; and confides in them so entirely as to persuade himself, against the voice of common reason, that a body may be drawn to a center by centripetal forces, where though the velocity is perpetually increasing, yet at the center the body shall at once by some unknown cause be entirely stopt †, or, which is more strange, annihilated ‖, and in other cases be turned directly back again **; that a resisting medium may be supposed endued with no other power, than that of retarding the body's motion, yet shall cause a body projected directly upward to deflect into a curve line ††. We find the author of the second proposing as discoveries, what has been the received opinion of many ages ‖‖; reviving maxims so deservedly exploded, that no man, who reflects, can hesitate about their falshood, whenever his eyes are open ***; deterred from attempting the solution of a cubic equation †††; exhibiting incon-

* Pag. 8 23.

† Pag. 11.

‖ Pag. 23.

** Pag. 11. 21.

†† Pag. 28.

‖‖ Pag. 56.

*** Pag. 42.

††† Pag. 55.

conclusive

conclusive demonstrations ||, and even unapprized of the meaning of so familiar a term as the similarity of figures*.

4. BUT how far these discourses are examples of the preceding reflection, I shall not presume to determine; nor yet what claim the writer on distinct and indistinct vision may have to mathematical knowledge of any kind, since he has not given any public specimen of great pretensions that way. Independent of these considerations as the first is intended for a very general explanation of motion, and the other promises no less than a compleat system of so extensive and interesting a subject as opticks; I hope these animadversions upon the failings, which I imagine myself to have found in them, will not be disapproved of. Since such general writings are usually had recourse to by those, who are first inquiring into any subject; it is not an unuseful labour to obviate the misinformation, to which learners by the errors in works of this nature must be exposed.

5. I shall also add some Remarks on the Essay upon distinct and indistinct vision subjoined to the compleat system of opticks, that the profuse praises bestowed on it by the editor may not occasion any one to be misled by the explanation there attempted of some of the principal points in Sir Isaac Newton's doctrine of light and colours, in which that great man's sense appears wholly unknown to this writer.

6. I have chose to deliver myself every where with great plainness, and without those apologies and excuses, with which writings of this kind do so often abound; as such affected complements upon these occasions have always given me offence in others. In relation to Mr. Euler, who is no otherwise known to me, than from his

|| Pag. 67. 72. 75. 77.

* Pag. 70.

works, though I have explained my thoughts without reserve; yet I hope free from any undue severity. In what concerns the other two gentlemen I have also made it my endeavour to avoid all excessive exaggerations of the faults, I have censured. But if any exceptionable expressions may have ever chanced to escape me, I trust, I shall easily be excused by them, who very well know the uncommon liberty, which has of late been taken with myself. To be more explicit, as the writer on distinct and distinct vision is the reputed author of the late dissertations under the name of Philalethes Cantabrigiensis, and the other gentleman is not only suspected of being his associate, but merited, no doubt, by singular services that high strain of complement, with which Mr. Faber is in one of those papers addressed; I hold myself under no farther restraint, where I have been used with such gross ill manners, than what a just deference for truth requires. I am thus free, not only because I think the treatment, I have received without the least provocation on my part, calls for some resentment; but also that I may do these gentlemen the justice to give them an opportunity of acquitting themselves, if they are falsely accused.

REMARKS

R E M A R K S

O N

Mr. LEONARD EULER'S

T R E A T I S E

ENTITLED

M E C H A N I C A.

1. **T**HE author of the first of the two books, I here intend to examine, was a scholar of the famous Mr. John Bernoulli, and has formerly published several pieces, which have met with such applause, that he has been invited to the new Academy of Sciences erected at Petersburg, where he now enjoys the honourable title of Professor of the Sublime Mathematicks. As this performance is composed on a very noble subject, which has so justly acquired to our great Sir Isaac Newton an unrivalled reputation; its merit has been celebrated in the various literary Journals of different countries with high encomiums.

N 3

2. THIS

2. THIS treatise after stating the general properties and affections of motion considers at large the effects both of impulsive forces, and of resisting mediums on moving bodies.

3. THE first proposition is, "That every body, which either by an absolute or relative motion is transferred into another place, must pass through all the intermediate places." This proposition is, I think, merely identical; the idea of passing through these intermediate spaces being necessarily and obviously included in the idea of motion.

4. THE third proposition, "That in any unequal motion the least elements of the space described may be conceived to be passed over with a uniform motion," is not universally true.

5. In many cases indeed the deductions founded on this position will be just, if we consider it as a figurative expression, meaning no more, than that the ultimate proportion of two contiguous spaces described in equal times by any variable motion is the proportion of equality. But where ever this interpretation cannot be applied, there this proposition will not only be faulty in its method of conception, but all the conclusions founded on it will also be erroneous. This will most remarkably be the case, when those spaces are compared together, which a body accelerated by any force describes in the beginning of its motion; for the ultimate proportion of the first of two contiguous spaces thus described in equal times to the second is not that of equality, but the ratio of 1 to 3, as is well known to every one acquainted with the common theory of falling bodies. This proposition will also fail, when applied to the estimating the increments of velocity produced in any moving body by any accelerating force; for if the velocity receives by the action of a uniform force equal additions in two equal contiguous portions of time, the additions to the spaces described

scribed in consequence of these increases of velocity will be as 1 to 3, instead of being to each other in the ratio of equality, as they ought to be, if this proposition were true.

6. MR. EULER in the annexed scholium has some obscure suspicions of the fallacy of this proposition: but as he applies it without scruple through the remaining part of his book, even in those instances, in which he here seems doubtful; it is evident, that the genuine principles, from whence alone all deductions of this kind receive their accuracy and rigour, were unknown to him; especially since some of his most capital errors arise from an unwarranted adherence to this proposition, as we shall more distinctly shew hereafter.

7. THE 7th, 8th, and 9th propositions relating to the continuation of a body in its state either of rest, or of equable motion in a right line, unless disturbed by some external force, ought to be proposed, as facts only; it being from experiments, and not from any metaphysical notions about bodies and motion, that the truth of these principles can be evinced. No wonder therefore, if Mr. Euler's pretended demonstrations of these propositions be altogether inconclusive and inconsistent.

8. IN the 11th proposition he is so little apprized of the relation, which a demonstration ought to have to a proposition, that here, though the proposition be general, the demonstration regards one particular case only, and the general proof is postponed to a scholium, in which he has so forgot his proposition, that he thinks it necessary to prove in form, that the ratio of a to b is given, which is the very thing, the proposition supposes. Nay more, in order to make this proof he has assumed a triangle to be given in species, merely because one angle is given. In order to prove that NL or BM will have to AL (fig. 9. Tab. I.) a given proportion, it is asserted, that the

triangle ALN is given in species; but this is not true, unless this proportion of BM to AL is first supposed to be given, nothing being determined in that triangle independent of that supposition, but the angle ALN.

9. MR. EULER's 14th proposition, very indistinctly proposed, contains in the solution there given, and the corollaries annexed no less than four capital errors.

10. FIRST it is said at the end of the solution, that the right line AD (fig. 1. Tab. II.) may be taken for the path described by the body, when acted on by a power in the direction AC. But this is false even on the confused principles of indivisibles. For were it true, the effect of the power in inflecting the body from its rectilinear motion, if estimated between D and A, would not be in the duplicate proportion of the times of its action, as it ought, but in the simple proportion of those times. The truth is, that every part of the motion, however minute, must be considered as a curve touching AB, and not as a right line intersecting it.

11. SECONDLY we are told in coroll. 2. that dc is equal to $\frac{Db}{dt}$, that is, to speak sense we are to conceive, that in the same time, in which the body with the velocity, it has at A, would describe the space AB, or the equal space Ab , it would with the additional velocity, it is augmented by at D, describe the space Db . But this is not true; for this increase of velocity is not acquired immediately in the point A, but during the whole time of the motion from A to B; and therefore, the ultimate relations being considered. Db is but half the space, that would be described during the passage from A to D by the increase of velocity at D; that is, in the infinitesimal jargon, dc is not equal to $\frac{Db}{dt}$, but

to $\frac{2Db}{dt}$. Mr. Euler has been led into this error by relying upon his 3d proposition quoted by him in the preceding corollary, which we have above observed to be a false one.

12. THIRDLY in the 4th corollary the same error occurs again, but in another form, being compounded with the cosine of the angle BAC.

13. FOURTHLY in the 5th corollary the angle of inflection of the direction of the body at D from the direction AB is determined to be the angle BAD, that is, the path described by the body is supposed to be the right line DA; but this is not true, as we have shewn above. Considering then the path described to be a curve touching AB in A and passing through the point D, as it ought, we must measure the inflection by the angle, that the tangent to this curve in the point D makes with the tangent AB, and this angle, ultimately considered, will be double BAD, that is, its sine in Mr. Euler's phrase is not $\frac{Bb}{BA}$, but $\frac{2Bb}{EA}$.

14. THESE errors now mentioned are variously compounded with each other in the 6th, 7th, 8th, and 9th corollaries; so that not only the general solution of this problem, but 7 of the annexed corollaries are altogether erroneous, and this even on Mr. Euler's own principles.

15. THE solution of the next proposition (prop. 15) deserves particular notice. Here AB represents the space, which a body with the motion, it has in A, would describe in the direction AE (fig. 2. Tab. II.) during any small interval of time, and Bb the space, which the same body would have described from rest in the same interval by the power acting in the same direction AE; that is, Ab represents the whole space described in some small interval by the uniform velocity of the body in A, and by the action

action of a power in the same direction. Now we are told, that in another equal interval by the velocity acquired in b uniformly continued it would describe another space equal to Ab . But this is false. For since the velocity of the body is perpetually changing from A to b , the space Ab cannot be conceived, as described by the whole velocity acquired at b , but by some velocity intermediate betwixt that and the velocity at A ; the space then, that would be described by the velocity at b in the next equal interval, will be greater than Ab , and if the action of the power was uniform from A to b , (as Mr. Euler by styling the interval of time an infinitely little one supposes) it will be equal to $Ab + Bb$; instead therefore of the equations in Mr. Euler $Ab = AB + ao$, $bc = AB + 2ao$, $cd = AB + 3ao$, $de = AB + 4ao$, there ought to be substituted $Ab = AB + ao$, $bc = AB + 3ao$, $cd = AB + 5ao$, $de = AB + 7ao$. All this has been so often shewn by every writer on the common theory of falling bodies, that I am surprized at Mr. Euler's mistaking so plain a matter. But these are not all the errors of this proposition. For Mr. Euler having by the application of his 3d proposition concluded, that the celerity of the body A is increased in the same proportion, as the increments of the spaces described by it, we may shew from the equations, we have rectified above, that taking this principle for granted, the increments of the celerity can never be proportional to the times, in which they are produced; for if the times are taken in the proportion of the numbers 1, 2, 3, 4, 5, &c. the increments of the spaces, and consequently of the velocities, if Mr. Euler is to be believed, will be in the proportion of 1, 3, 5, 7, 9, &c. a proportion not only different from that of the times, but, if the extrems are compared, perpetually receding from it. Another principal error then of this proposition is, that notwithstanding these erroneous

roneous suppositions the conclusion, "that the increments of the celerities are ultimately as the times, they are produced in," is indisputable, as has been frequently shewn by other writers. Now it can be only by a complication of errors, that true deductions are ever derived from false principles.

16. THE process annexed to the 18th proposition is absolutely inconclusive, and unworthy the name of a solution.

17. AS Mr. Euler tells us in the 2d corollary of the 19th proposition, that he has been particularly solicitous about the rigid demonstrations of these last propositions; I have no design to charge this author with haste or negligence on account of these errors; but I consider them solely, as the effect of that inaccuracy in conception, to which the differential calculus is disposed to betray its admirers.

18. IN the solution of the 21st proposition and its 1st corollary all the errors of the 14th are continued; and consequently all the equations there given are on that account false. But in the 2d corollary Mr. Euler has fallen into an error of still a stranger nature, even following the principles of his differential calculus to an express contradiction of a proposition of Euclide's elements, which shews, that the angle at the circumference of the circle subtended by the line AD (fig. 6. Tab. II.) not the angle at the center, is equal to the angle BAD; and consequently it is not the triangle AOD, OD being supposed to be drawn, but the triangle formed on AD by lines drawn from that point, where AO continued meets the circumference beyond the center, that is similar to the triangle BAb; and therefore Bb is not to AB as AD to AO, but as AD to 2AO. It is not then the radius AO of the circle touching the line AB and passing through the point D, but the diameter of that circle, which Mr. Euler has here assigned, that
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is, $2AO$ is $= \frac{AB \times AD}{Bb}$, not, as he makes it, $AO = \frac{AB \times AD}{Bb}$.

19. THIS determination of the radius of curvature in the present corollary is, as it were, the basis, on which the future examination of all curvilinear motion does immediately depend; and were not this error compensated by those, we have taken notice of already, and others, that we shall hereafter remark, there could not be one proposition in all this volume, where the body is supposed to move in a curvilinear track, but what would be erroneous. This proposition is indeed of such necessary use in the methods of proceeding followed in these volumes, that it occurs perpetually; but its falshood no where more eminently appears, than in the demonstration of the 1st proposition of the second volume, where it is more obvious, and more express, if possible, than in the present instance.

20. THE value of r then assigned in the 2d corollary, and used in the succeeding ones of this 21st proposition, is double, what it ought to be.

21. BUT in the 10th corollary this erroneous value of r is compounded with a value of dc , which we have shewn above to be but one half of what it ought, and by this means the equation resulting in this corollary is rectified; the two errors ballancing each other.

22. THE sense of the 22d and 23d propositions may be briefly exhibited thus.

23. LET a, b, c, d , be parts of the body O considered as a point, and OA, OB, OC, OD , (fig. 8. Tab. II.) a like number of forces applied to that point, and let OG be the force compounded of all these. Then, if the parts a, b, c, d , are supposed to be separately accelerated by the respective forces OA, OB, OC, OD , I say, that the motion of the common

common center of gravity of these parts a , b , c , d , will be in the direction OG, and with the same acceleration, as would be that of the entire body O, if accelerated by the force OG.

24. THIS is the true and genuine form, under which, I think, this proposition should have been expressed: the principle of a restoring force, which Mr. Euler has introduced, serving only to perplex the reader's imagination, and render the proposition unintelligible.

25. THE demonstration is equally inaccurate; for in the first place it is no demonstration of Mr. Euler's proposition (of which indeed no demonstration can be made, as it contains no assertion, that is the proper object of demonstration,) and it is in the next place excessively artless and tedious, containing above 20 equations, when the whole may be easily dispatched by the application of a well-known proposition in statics, without any computation.

26. IN the beginning of the third chapter, which treats of right-lined motion, Mr. Euler has given Galileo's theory of falling bodies, in its own nature no difficult subject; but it is here so compounded with differential computations, that this subject may be much better learned from what has been writ in a more simple manner by others.

27. IN the remaining part of this chapter he treats in general of the direct ascent or descent of bodies accelerated by any forces referred to a given point. And in his 1st scholium to his 32d proposition he is much perplexed how to determine the motion of the body, when it has passed the point C (Tab. II. fig. 13.) He apprehends no more necessary than making y negative in the expression of the solution given in the proposition; and thence concludes, that if the quantity resulting is affirmative, the body will really pass the center C, but if it be negative, it is an indication, that the body will never pass
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the point C. He however owns, that in some cases this method of prosecuting the motion includes a contradiction.

28. BUT though he has concluded, that in certain instances this proceeding will mislead us, because the result in those cases is different from what, he knows from other principles, it ought to be; yet instead of making the true use of this discovery, and suspecting the whole process, he still supposes the solution to be right in those cases, where he has not the same means of comparing the result with known principles.

29. NOTWITHSTANDING he discovers by the absurdity of the conclusion, that the method of solution, he has given, will in numberless instances fail in determining the motion of the body, when it has passed the center, he nevertheless in other instances, where the same absurdity occurs, supposes the motion of the body to be rightly exhibited by these fallacious operations; having advanced in the 1st and 2d scholium, and 5th and 6th corollaries the three following wonderful positions.

1st, THAT in many cases neither the motion nor direction of the body after it arrives in C, can be determined.

2d, THAT in other cases the body, after it arrives in C, can pass no farther.

3d, THAT in another case there mentioned the body, when it arrives in C, will instead of proceeding forwards fly back again in the same direction, in which it fell.

30. IT is true, our author speaks of all these determinations with some diffidence and uncertainty, and seems to think them very mysterious; but he satisfies himself with a blind submission to his computation, "*quicquid autem sit hic calculo potius quam nostro judicio est fidendum.*" Nay in the last conclusion he seems even to have satisfied his judgment;

judgment; for in the passage, he refers to in the following part of his book, he has copied Sir Isaac Newton, as he conceives, and not understanding his author seems to suppose himself sheltered under his authority.

31. Now the whole mystery, which has thus embarrassed our author, is this; When y , the distance of the body from the center, is made negative, the terms of the problem are sometimes changed with it. The centripetal force being as some power of the distance expressed by y^n , where n may be any number affirmative, or negative, whole number or fraction; if, when y is supposed negative, y^n be still affirmative, the solution gives the velocity of the body in its subsequent ascent from the center; but if y^n by this supposition becomes also negative, the solution exhibits the velocity, after the body has passed the center, upon condition, that the centripetal force become centrifugal; and when on this supposition y^n becomes impossible, this determination of the velocity beyond the center comes out impossible, the condition being so.

32. IN the corollaries and scholium annexed to the 48th proposition, which considers the ascent of bodies from a center, he meets with the same difficulties, and is at great pains to account, how the solution, he has there given, should fail him in certain cases; and to satisfy himself he recurs to infinitely small, infinitely great, and other the like ambiguous phrases, the usual modern palliatives of error. But so little does he seem convinced with his own reasoning on this head, that in the 96th section of his second volume, he has in a similar case supposed the computation to be accurate; disregarding those objections, he here makes use of to prove it fallacious.

33. THAT the computation in this proposition is unskilfully applied, cannot be doubted of at first sight;

fight; since, when n is any affirmative number, the force in C absolutely vanishes; and consequently the body will in these cases remain for ever at rest in the point C, if once placed there. Whatever computation therefore pretends under these circumstances to exhibit both the time of passing from the very center through any given space, and the velocity by that means acquired, must undoubtedly contain under it some fallacy. The truth is, the time determined by this method is the limit only of that time, in which the same given space would be described, supposing the body not to begin its motion from the very point C, but to be placed at first any where short of it. Indeed whatever be the centripetal force, if it diffuse itself every way equally from the center, no body can be moved out of the center by it.

34. THE problem proposed by Mr. Euler in his 43d proposition is from the relation of the times of description to the spaces described to determine the law of the impulsive force.

35. HERE he is entangled in difficulties from not understanding, that the conditions of a problem often put a limit upon algebraical expressions. This Sir Isaac Newton has very well illustrated in his treatise of algebra by the following easy instance*. If it were required to inscribe any chord given in magnitude within a given semicircle; x denoting the versed sine of the arch, which will be subtended by the given chord, a the diameter of the circle, and b the chord, x will be $= \frac{bb}{a}$: therefore whatever be the given magnitudes of b and a , there may be always found a value of x , which will satisfy the algebraic equation: yet it is manifest that if b be greater than a , the value of x derived from the equation will be of no use for solving the problem, which in

* P. 245.

that

that case is impossible. In like manner Mr. Euler proposes a case of his problem, (in the scholium) where the time of the body's motion shall be expressed by $\sqrt{2ax-xx}$, x denoting the space, through which the body has moved, and a some given length. Here he perceives, that this expression cannot in all magnitudes of x express the time of the body's motion; for when x is greater than a , the quantity $\sqrt{2ax-xx}$ diminishes, whereas the time already passed cannot be recalled.

36. BUT what he gathers from thence, is absolutely absurd; that when the body is arrived at C, it must ever remain in that point, which he owns himself is past all conception; nay more, that the velocity of the body, if it passed farther ought to be negative, which how to account for, he confesses himself unable. Indeed all, that can be performed in this problem, is to find such an impulsive force, wherein the given expression shall for some space denote the time. And when this is discovered, the limits, to which this expression must be subject, will be known. Here it comes out, that the impulsive force will be reciprocally as the cube of the distance of the body from a point at a distance equal to a from the place, whence the body set out. After this is known, it is easy so to express the time, that the expression shall agree to the body's whole motion. The time of the body's passing between the center now found, and any other place, will always be denoted by $a - \sqrt{2ax-xx}$.

37. IN the following chapter Mr. Euler treats of right-lined motion in a resisting medium.

38. HERE in the scholium annexed to the 20th definition we are told, that all other laws of resistance, except that which observes the duplicate proportion of the velocity, are merely imaginary; which is also repeated in the 1st corol. of prop. 50. This is so great a mistake, that no medium is yet known, in

which the whole resistance is accurately in that proportion. Our author had not attended to what, Sir Isaac Newton expressly takes notice of, that in all fluids some share of their resistance, though much the least, is owing to some degree of tenacity between their parts, and that this resistance is the same in all degrees of velocity.

39. IN the 4th corollary of the 50th proposition our author tells us, that when $m > 1$, the body, before it arrived at A (Tab. IV. fig. 2.), had somewhere, as in C, an infinite velocity; the meaning of which strange assertion is no more, than that a body, which should set out from the point C, determined in the manner there laid down, with any velocity, however great, would lose all its motion short of the point A; and consequently could never reach that point.

40. IN the third scholium of the same proposition Mr. Euler seems again surprized, that his computation should give an expression for the velocity, when in reality there can be no motion; but we have already explain'd this difficulty in our observations on the corollaries of his 40th proposition, and shall therefore say no more of it here.

41. AFTER the 53d, 54th, 55th, 56th and 59th propositions of Mr. Euler have been so excellently solved by Sir Isaac Newton, in the 8th and 9th propositions of the second book of his Principia; the weight of computation, with which Mr. Euler has filled them, can, I think, be no otherwise excused, than by supposing him unskilled in Sir Isaac Newton's treatise of Quadratures, to which recourse ought always to be had, in the analysis of such problems.

42. IN the solution of the 61st proposition, we have this equation $dx = \frac{kmdv}{gkm - vm}$, x representing the space passed through, and v a quantity in the duplicate

cate proportion of the velocity acquired at the end of that space, k and g being known quantities; and we are told, that by the quadrature of curves (ope quadraturarum) v may be determined by x , however in the annexed scholium Mr. Euler dismisses this equation from any future examination, because its integral cannot be exhibited. But by the improvements the late Mr. Cotes has made to Sir Isaac Newton's doctrine of Quadratures, which were soon after their publication demonstrated by Dr. Pemberton, it is always possible by the circle and hyperbola to assign the relation of x and v , whether m be an integer or a fraction; and therefore a more ample prosecution of this equation might reasonably have been expected from a person so much in love with computation.

43. IN the first scholium of the 62d proposition we are told, that the velocity of the moving body always vanishes at C, because the resistance in that point is infinite. But this is false reasoning; since every infinite resistance will not instantaneously destroy a finite velocity. For example, should the density of the medium be reciprocally in the subduplicate proportion of the distance from C, and the resistance in the simple proportion of the velocity, which will be the case in this proposition, when m denotes the fraction $\frac{1}{2}$; notwithstanding such a resistance, the body may yet arrive at C, with any given degree of velocity however great.

44. IN the second scholium to the 66th proposition Mr. Euler observes, that the equation determining the quantity q in that proposition will not be changed, although instead of p or R any multiples of those quantities are taken. And as the magnitude of q in reality varies, when instead of R any multiple is taken; he accounts for this only by telling us in general, that a differential equation is more extensive than the integral, from whence it is deduced.

45. THIS vague and indetermined reason shews, why Mr. Euler considered this as a difficulty; which could scarce appear so to one duly apprized, that the fluxion of a logarithm of any quantity is the fluxion of that quantity, divided by the quantity; therefore whatever given number or quantity multiplies the quantity proposed, the same multiplies both the numerator and denominator of the fluxion, whereby the fluxion remains the same, however that number or quantity be varied; as it ought to do, because the multiplying a flowing quantity by any given one only increases its logarithm by the logarithm of the given quantity.

46. IN the fifth chapter, which treats of curvilinear motion, every proposition from the beginning to the end does immediately or consequentially depend on those erroneous equations, which we have taken notice of in the remarks on the 14th and 21st propositions; and therefore, however true the conclusions here given may be; yet, as they are founded on wrong positions, they owe their coincidence with truth to chance only, by the accidental intervention of contrary errors.

47. BUT admitting the principles here made use of to have been truly and accurately obtained; yet there are other important exceptions both to the method of this chapter, and to particular conclusions contained in it.

48. IN relation to the method of investigation here made use of, it is necessary to observe, that the ancient geometers in their analysis, no less than in their composition, brought in aid, as occasion required, whatever had been discovered by others; and that investigation was by them the most approved, which contained in it no process, that could be supplied or contracted by any known proposition. On the contrary our algebraical gentlemen in the modern taste lay aside all regard to order, conciseness,

ness, or elegance in their conceptions; and confine their whole labour to the single ambition of rendering all previous knowledge as little necessary, as possible, to their present conclusions.

49. MR. Euler has here so closely pursued this scheme, as not to think of digesting even his own subject into any method, which might facilitate his analyses. Instead of premising these two general propositions, That of the equality of the areas described in equal times about the center of attraction, and That of the equality of the velocities at equal distances from that center both in the direct and curvilinear fall, whereby his following analyses might have been abbreviated; he plunges without preparation into compound propositions, and contents himself with slightly deducing these, as corollaries, from a proposition [prop. 74.], to which they should have been premised.

50. Two such propositions, which are or ought to be the very basis of all investigations of curvilinear motion, certainly merited to have been particularly discussed in their full extent, instead of being thus derived, as it were, by chance, from a proposition not so extensive as they are. This is inverting the natural order of the subject, and necessarily involves it in obscurity and perplexity.

51. BUT to be more particular, the deductions in the 70th and 71st propositions needed not that pomp of differential equations and canons; being easily deduced with more brevity without them, particularly the 3d and 4th corollaries of the 71st.

52. IN the 72d proposition, and its corollaries the effect of gravity compounded with a rectilinear motion is discussed with a degree of intricacy even surprising in so simple a subject.

53. THE 73d, a very easy subject, is not much better treated.

54. IN the 74th proposition the radius of curvature is unnecessarily involved in the computation, to no other purpose than that of embarrassing the process, till it is exterminated.

55. THE solution of the 75th is unmeasurably prolix; for if from the center C a circle be described, whose radius is unity, u will in this circle be the cosine of the angle MCP, and the ultimate proportion of the increment of CM or y to the increment of u will be compounded of the ratios of MT to TC, CM to the radius 1, and the radius to the sine of the angle MCP, that is, of the ratios MT to TC, and CM to $\sqrt{1-uu}$; whence the differential equation

$$\frac{du}{\sqrt{1-uu}} = \frac{Ody}{y\sqrt{yy-QQ}}$$

is immediately deduced; to arrive at which Mr. Euler has employed near 20 equations, substitutions, restitutions, &c.

56. BESIDES the general obscurity, in which the 78th, 79th, 80th, and 81st propositions are here involved; there is in the 11th corollary of the 80th an error of so extraordinary a nature, as to merit a particular discussion.

57. IF the attraction directed to any center be reciprocally in the duplicate proportion of the distances from that center, and a body be projected from any point in a direction perpendicular to the line joining the center and that point with a certain degree of velocity; it is then confessed, that the curve described by this body will be an ellipsis, the center of attraction being its focus, and the point, from whence it set out, becoming that extremity of the axis, which is most removed from the focus. It is allowed too, that by diminishing the velocity of the projected body the species of this ellipsis is perpetually changing, approaching more and more to a right line; and that the time of half a revolution in this reduced ellipsis perpetually approaches to the time of the direct fall to the center. And since in these reduced ellipses
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the body, after it arrives at its lower apsis, will perpetually return back again to the point, from whence it was originally projected; since, I say, this will be true, however small the velocity is, with which the body is originally projected, that is, however near the species of the ellipsis approaches to a right line: Mr. Euler has falsely concluded, that in the direct fall, when both the original velocity, and the ellipsis vanish, the same return will take place, that is, that the body, the moment it arrives at the center, instead of proceeding forwards with the velocity it has there acquired, will immediately return back towards the point, whence it fell.

58. Now though this monstrous supposition obviously manifests its own absurdity, and carries its own confutation with it to every one, that at all considers it; yet that Mr. Euler may not be confounded in searching for those erroneous deductions, which have here misled him, I will inform him, that the reason, why, when there was any transverse velocity originally impressed, however small, the body constantly returned, after it was got at some small distance below the center of attraction, is this, that then the attraction to the center never conspiring in direction with the motion of the body, the direction of that motion by this means perpetually changes, till below the center, where it acquires its greatest velocity, it becomes perpendicular to a line drawn to the center, and consequently returns from that point with a motion altogether similar to that, with which before it descended. But if there is no such original velocity, and consequently no obliquity, this reasoning no longer holds good; for the body will now be ever found in the right line passing through the center; and therefore, when it has once acquired a progressive motion in this line, it must continue it, till having passed the center its velocity is abolished by the contrary action of the centripetal force.

59. IF the reader, after the numerous mistakes which this author has been guilty of, in relation to the perpendicular descent of bodies, is desirous of seeing them all in epitome; he need only consult the 2d scholium of the 91st proposition, which will fully satisfy his curiosity, and afford him an ample field for astonishment. He will there find our author's former discoveries even improved; that bodies drawn to a center by centripetal forces, which increase perpetually their velocity, shall not only on a sudden be stopt at the center in the midst of their swiftest motion; but be moreover annihilated*.

60. IN the demonstration of the 98th proposition we are told, that the radius of the curvature (Tab. IX. fig. 4.) at the point m , is $\frac{mv^2}{vE}$. But this is the old mistake of taking the radius instead of the diameter. For, since the line mn is a tangent in the point m , and V a supposed point of the curve, $\frac{mv^2}{2vE}$, and not $\frac{mv^2}{vE}$, is the radius of the curvature at that point; as we have formerly shewn in our remarks on the 2d corollary of the 21st proposition.

61. IN the 4th corollary to this proposition the second differences of z and y are falsely assigned, being there but of half their proper magnitude. For to authorize the conclusions in this corollary Mm ought to be the chord of the preceding arch continued; but since it is supposed to be the line, in which the body would proceed forwards from m , if not acted on, it must of necessity be considered as a tangent to that point m .

62. THE remaining part of this chapter is excessively obscure, tedious and ten times more com-

* Corpus, statim ac in centrum pervenerit, ibi evanescet.

pounded, than it need to be, of which take the following examples.

63. THE 101st proposition requires none of those complicated equations, it so much abounds with. The true method of solution is to conceive the motion of the body to be compounded of two motions, one of them parallel to the axis, and the other performed in a plane perpendicular to the axis; then considering these separately, the motion parallel to the axis will be ever uniform, being no ways changed by the attractive force; and the motion in the plane perpendicular to the axis will be reduced to the simple contemplation of the figure described by a body acted on by a known central force, and having its direction in the same plane with that force; then by combining these two motions together the situation of the body may at any time be known. From this method of solution the whole is reduced to principles already determined, and the conclusions in the two examples annexed to this proposition are attained without any computation.

64. IN like manner the 102^d proposition abounds with a complication of differential equations no ways necessary. For if from the point A in the figure of that proposition a line be drawn perpendicular to the plane APQ, let us denominate this line the axis. Then the force in AM may be always divided into two others, one of them parallel to this axis, and the other at right angles to it. But the parallel force being in the same direction with the force MQ, the body by the conjunction of these two forces will approach the plane APQ, and from the known quantities of these forces, and the known velocity towards this plane, the motion of acceding to this plane will be determined by the third chapter, it being under these circumstances a rectilinear motion: and by the force perpendicular to the axis a curvilinear track will be described round that axis in a plane,
and

and to which that axis is always perpendicular ; and as this curvilinear orbit may be determined by the consideration of the motion in a plane only, it is evident, that by this method the whole motion of the body will be determined by the means of known propositions, without the assistance of new principles, or the perplexity of new computations.

65. OF the superiority of this method of solution we will give the following instance.

66. SUPPOSE a body at M to be attracted partly to the point A (Tab. II. fig. 1.) by a force directly as MA, the distance from that point ; and partly to the plane APQ by a force directly as MQ, the perpendicular distance from that plane.

67. To determine this body's motion let the force AM be divided into two forces AQ, QM, one of them, as AQ, perpendicular to the axis, and the other QM parallel to it. The first of these will be directly as the distance from the axis, since AM is always as the force directed to the point A, and by this force there will be described in the plane, to which that axis is perpendicular, an ellipsis having the intersection of the axis with that plane for its center. The other force parallel to the axis will be as MQ, the distance of the body M from the plane APQ, but the original force, by which the body M was attracted to the plane APQ, was also as this distance MQ ; therefore the whole united force, by which the body M is urged towards that plane, is as MQ ; the motion then of acceding to that plane is the same, as that of the direct fall of a body towards a center, to which it is attracted in proportion to its distance from that center. Therefore the motion of the body in a plane perpendicular to the axis being known, and the motion of acceding in lines parallel to the axis to the plane APQ, or receding from it being known, the whole motion of the body is thereby determined.

68. FROM

68. FROM the known proportions between the force on M acting parallel to AQ, by which the ellipsis is described, and the separate forces, that by their conjunction in the direction MQ urge the body towards the plane APQ, the proportion between the time of one revolution in the ellipsis, and the time of one oscillation in a direction perpendicular to the plane APQ is easily assigned; and from hence the change of the position of the nodes, or of those points, in which the body passes through this plane in each revolution, is determined, the time of the bodies passing from one node to its opposite being the time of one oscillation. If therefore a line be supposed drawn from the center of the ellipsis to the node, in which the body was last found, the next position of the node will be determined by a line drawn also from the center of the ellipsis, containing with the first drawn line an area, that shall be in such proportion to the whole ellipsis, as is the time of one oscillation to the time of one revolution in the ellipsis.

69. THIS instance is the same, with the example Mr. Euler makes use of at the end of his 102d proposition (only an error of the press in the first line must be rectified, where instead of AP there is MP,) it also includes his 103d proposition, which is the determination of the nodes in this case. In the solution of this example Mr. Euler has filled several pages with the most abstruse and complicated computations. There are equations arising to the 7th, 8th and 9th powers, and involving the third power of first differences, and the second power of second differences; there are introduced logarithms both real and imaginary; nay the simple determination of the nodes cannot be dispatched without the relation of tangents and sines to their respective arches.

70. THE last chapter of this volume, which treats of curvilinear motion in a resisting medium, is equally

ly exceptionable with any of the preceding ones in respect to its method, and the operose computations, with which it is unnecessarily filled. But as we have already distinctly examined other parts of the book with regard to these particulars, we shall not enter into a farther discussion of them in this place; we shall only conclude our remarks upon this volume by obviating a most erroneous determination of the motion of a body in a resisting medium, in the scholium annexed to the 112th proposition.

71. IN this proposition Mr. Euler inquires, what law of resistance combined with the uniform force of gravity acting in the direction AC (fig. 6. Tab. XI.) will compel a body to move in a given semicircle, as BAMD.

72. THE determination is, that, if the resistance in every point, as M, is to the force of gravity, as three times the distance MQ to twice the radius AC, and this resistance is supposed to conspire with the motion of the body, when it moves upwards in the quadrant BA, but to be oppos'd to it, when it moves downwards in the quadrant AD; then the body by the combination of this resistance with the force of gravity, will describe the semicircumference BAD; and from hence in the scholium he concludes, the resistances in the quadrant AD being equal and altogether similar in their directions to those in the quadrant AB, that the body, when it arrives at D, will by means of these resistances already established return back through the semicircumference DAB to B.

73. THIS appears even to Mr. Euler wonderful, that a body at rest in D, and acted on by a force perpendicular to the line DC, for in this direction does the resistance act at the point D, should by means of this force be impelled in a line different from that perpendicular: but he thinks, he has solved the difficulty by telling us, that the force acting in the direction of the semicircumference at the point B is not
abso-

absolutely perpendicular to the diameter in that point, but has an infinitely little deflection from the perpendicular.

74. WHAT the angle measuring this infinitely little deflection is, Mr. Euler will never be able to explain; it being an unintelligible reason urged in support of a false fact. For when the body arrives at the point D, it cannot return in the circumference DMA, but by the intervention of some superadded force. What has here misled Mr. Euler, is his want of discerning, that the determination of the law of the resistance supposes the body, when its resistance is investigated, to move with a motion oblique to the action of gravity; it being only by means of this obliquity, that the comparison of the resistance with the force of gravity can in this case take place. And consequently the points B and D are excluded from all inferences formed on these principles; no more being determined, than that by such resistances, as are here assigned, the body will move from any one point of the semicircle to any other, provided the point, from whence the motion is supposed to begin, is neither B nor D.

R E M A R K S

O N

Dr. S M I T H's

C O M P L E A T

S Y S T E M of O P T I C K S.

I. **I** SHALL at present proceed no farther in my remarks on Mr. Euler's book; as I apprehend, what has been said, will be sufficient to prevent an intelligent reader from being misled by any of the mistakes of the succeeding part. And, I think, it appears, that the most of this gentleman's errors are owing to so strong an attachment to the principles, he had imbibed under that inelegant computist, who was his instructor, that he was afraid to trust his own understanding even in cases, where the maxims, he had learnt, seemed to him contradictory to common sense. But now I am to enter upon a more arduous undertaking, to pass censure upon the labours of a gentleman, who from his being a relation of the late most excellent Mr. Cotes, not only possessed all his papers at his death, but during his life enjoyed so uninterrupted a conversation with that great man, as furnished him with an opportunity

nity of every kind of information, he could at any time desire. Therefore as we gather from Mr. Professor's proposals, that he has been employed more than twenty years upon this work, and as he has informed us, that Mr. Cotes himself before his death had the same subject under consideration; it were reasonable to expect no small share of perfection in such a performance. I shall therefore endeavour to proceed in my remarks with all the circumspection, which these reflections may seem to require.

2. THIS treatise sets out * with a very unskilful representation of a capital proposition in Sir Isaac Newton's Opticks†, by omitting an essential part of it, which Sir Isaac Newton had expressed under the form of an exception. For this exception is so extensive, that the refractive power of water, the most common of all refrangible substances except the air, is almost half as much again in proportion to its density, as that power is in the air, in glass, and other terrestrial bodies. This power in all the oils is more than twice as much as in glass, and in one substance, the diamond, near three times as much. The repeating the proposition afterwards at art. 189. entire, is no excuse for omitting the exception in this place; since the conclusion, which Sir Isaac Newton makes from this proposition, depends absolutely upon that exception.

3. THERE soon follows, at art. 17, another instance of this author's imperfect knowledge of the true theory of the action between light and bodies, where it appears, that when a ray within a denser medium falls upon the surface of it, he did not apprehend, that it would be reflected till such times, as the sine of incidence bore a greater proportion to the radius than that, which measures the refraction; whereas the ray will be reflected, as soon as the sine

* Artic. 5.

† B. II. prop. 10.

of the incident angle comes to that proportion. Upon this mistake he proceeds afterwards in the mathematical part, art. 472, 487, and talks of rays emerging in the tangent of a circle.

4. **THOUGH** in this popular part of his treatise the author might be allowed to omit some circumstances, which in mathematical demonstrations could not be dispensed with; yet he ought never to have expressed himself, as he has done at article 21, where he describes the rays, which in opticks may be considered as parallel, to be such, in which the difference of their distances at any two given places is insensible: whence it should follow, that the rays of the sun are not to be considered, as parallel, unless their distances at the orbit of mercury and at the earth were not sensibly different.

5. **IF** in the figure referred to in article 32, O mean the place of the eye, for which it is used in the fourth chapter, the assertion in this article is not true. For the rays, which from P enter the eye, will none of them proceed from *p*, as here determined.

6. **IN** article 43 the ray, which within any glass lens makes equal angles with each surface, is said to pass through the middle point of the glass; whereas in all concavo-convex glasses that ray is inclined to a point in the axis, which is always without the glass. This oversight is indeed attempted to be corrected in the mathematical part, Art. 228; but very imperfectly. It is only said, that in these glasses this point will be a little without the glass; whereas in such there is no distance, which it may not exceed. For the thickness of the glass is to the difference between the semidiameters of the spheres, of which the two surfaces are portions, as this distance to the lesser semidiameter.

7. **IN** article 95th this author has endeavoured to give such a proof of the proportion, which he
has

has assigned between the light, we receive from the moon, and that we receive from the sun, as he imagined, might be easily apprehended; though he informs us, that he discovered that proportion by other means. It follows, as far as it is just, directly from the proposition for the same purpose, of that excellent geometer James Gregory, in his *Geom. Par. Univers.* page 144; and the general proposition there mentioned for determining the proportion between the degrees of light received from any planet, and from the sun, is repeated in David Gregory's *Astronomy**, a book in every one's hand. When this author was comparing moon-light with day-light, had he described them both in a similar manner, as was reasonable to have been done, he would have seen, how little the description, he has given of day-light, is to the purpose; but it likewise appears from the result. For he first informs us, that the proportion, he has set down, is not true, but upon supposition, that the moon reflects all the light, it receives from the sun; and yet his argument is drawn from comparing the light of the moon, seen in the day, with the light of the clouds, that is, is deduced from the quantity of light actually reflected by the moon.

8. AS this gentleman at art. 98th, &c. in relation to the apparent place and magnitude of the images of objects seen by reflection and refraction, has thought proper to depart from the principles delivered by the most approved writers on opticks; in order to form a judgment upon this innovation, I shall first set down distinctly the received opinion.

9. THE figure and colours of objects are immediately presented to the sight by the images made upon the bottom of the eye; but their distance is

* Lib. 3. prop. 58.

judged of by concomitant circumstances. This amongst other means is naturally suggested to us by the rangement of the several objects, that occur to our sight; those appearing the most remote, where the greatest number of other objects are perceived to intervene. This way of receiving information of distance by the sight has been anciently taken notice of; being at large insisted on by Alhazen the Arabian in his treatise of Opticks, Lib. 2. n. 39. In extensive views there is also another more definite circumstance, well known to the painters, which suggests different degrees of distance. This is the blueish tint, which the colours of objects receive from the interposition of the air between them and the sight; which is greater in proportion, as a greater quantity of air is interposed between the eye and the object. But as the last of these two causes can have no sensible effect in very small distances, where the eye is found to form an exact judgment, even without the assistance of the other, it is supposed, that the eye is possessed of some powers within itself, whereby it receives this information. When any object is viewed attentively, we always turn the sight directly to it, that the axis of the eye be pointed to the object; and therefore in viewing an object with both eyes, the axes of the eyes are more inclined to each other, when the object is near, than when it is remoter; and this being an animal action, is capable, in consequence of habit, of suggesting to the mind the different distances of objects by the different degrees of this conversion of the axes towards each other. Besides, as it is found, that the eye cannot see objects at the same time distinctly, which are at any considerable difference of distance, but can alternately fit itself to either; this change made in the eye is also by habit capable of suggesting distance. Both these causes are very well explained by James Gregory, in his *Optica Promota*. At the
end

end of prop. 28, he thus expresses himself, "quoti-
 " diana experientia majoris vel minoris contorsionis
 " [axium oculorum] natura est edocta, ut de di-
 " stantiâ seu loco visibilis conjecturam facere pos-
 " sit;" and of the second in the next proposition,
 " naturæ insitum est, ut humores oculi fluidos &
 " mobiles aliquo modo disponat ad radiantia pro-
 " pinqua, ut distincte pingantur in oculi retinâ; ex
 " quorum humorum mutatione magna vel parva
 " dignoscit natura (quotidianis experienciis edocta)
 " num parvo vel magno intervallo distet radians
 " ab oculo; atque ita de illius loco conjecturam
 " facit." The first of these two causes was known
 before the latter, which was not apprehended, till
 after the manner of vision by the refraction of the
 eye had been well considered; insomuch, that Kep-
 ler, who has explained the first of these causes di-
 stinctly *, confines the judgment of distance from a
 single eye to those distances, which bear a sensible
 proportion to the breadth of the pupil †: and A-
 guilonius, who also admits of the first, lays down a
 proposition in form (though contrary to experience)
 that one eye only is incapable of judging of distance ‡.
 That the first cause does operate to this effect, is
 from hence concluded, that we judge better of di-
 stance by both eyes than by one alone, as is usually
 experienced by directing a person to endeavour to
 touch an object at some small distance by a lateral
 motion of the finger with one eye shut. But it is
 also evident, that the single eye will likewise in some
 degree direct to the distance; because, if the object
 is not touched, the finger will not pass at a great
 distance from it; and those, who have the use but
 of one eye, will generally succeed in the experiment.
 In optick instruments, where the images of objects

* Paralipomen. in Vitellionem, prop. 8.

† Ibid. prop. 9.

‡ Optic. Lib. 3. prop. 1.

are viewed with one eye, this last cause is supposed to be that, which suggests the distance of those images; and that the eye sees them in that place, whence the rays proceed, which enter the eye from the several parts of the image.

10. THESE are the most direct means of our judging of distances. But as we do not always look at objects with the greatest attention, there are other circumstances accompanying objects, which offer themselves to our sight, whereby their distance is suggested. In particular the angle, under which an object appears, whose magnitude is supposed known, will give us an apprehension of a greater or lesser distance, according as that angle, or more properly the magnitude of the image made in the eye, is less or greater. Likewise if bodies are in motion, when the angle, under which they appear, increases, we shall judge the body to approach, or recede, when that angle diminishes. These and other collateral means of our receiving suggestions of distance, when they do not conspire with the former, often occasion deception in the sight, and cause us to mistake the real distance of objects.

11. THE same deceptions are also liable to take place in the images of objects seen by optick instruments; and the experiments, by which this author was induced to depart from the received opinion, are evidently of this kind; where the image of the object by its increasing or diminishing upon the motion of the eye or glass seems to approach or recede, contrary to what it ought to do upon the common principles*. But this gentleman not duly considering, that the eye was thus equally liable to deception in optick instruments, as in direct vision, has been so far confounded by these appearances, as to think it necessary to relinquish the common doctrine, and

* Art. 138. and the remarks upon it.

to advance in its stead a new rule for determining the apparent place of objects seen in those instruments.

12. ACCORDING to this gentleman, the image of the object is always judged to be at that distance from the eye, where the object itself would be seen under the same apparent magnitude *. But this rule is in the first place deficient; as it can be no guide to the eye, when the original object has not first been seen. It is also contrary to fact in the most common and simple cases. In microscopes it is impossible, that the eye should judge the object to be nearer than the distance, at which it has viewed the object itself, in proportion to the degree of magnifying. For, when the microscope magnifies much, this rule would place the image at a distance, of which the sight cannot possibly form any opinion, as being an interval from the eye, at which no object can be seen. Yet this gentleman expressly asserts, at article 141, that the apparent distance of the image of an object seen either by reflection or refraction, is to the distance of the object seen by the naked eye, reciprocally as the apparent magnitude of the object to the apparent magnitude of the image. I believe, in general, whoever looks at an object through a convex glass, and then at the object itself without the glass, will find the object to appear nearer in the latter case, though it be magnified by the glass; and in the same trial with a concave glass, though by the glass the object be diminished, it will appear nearer through the glass than without. But the most convincing proof, that the apparent distance of the image is not determined by its apparent magnitude, is the following experiment. If a double convex glass be held upright before some luminous object, suppose a candle, there will be seen two images, one erect and the other inverted. The

* Art. 139.

first is made simply by reflection from the nearest surface, the second by reflection from the further surface, the rays undergoing a refraction from the first surface both before the reflection and after. If this glass has not too short a focal distance, when it is held near the object, the inverted image will appear greater than the other, and also nearer; but if the glass be carried off from the object, though the eye remain as near to it, the inverted image will diminish so much faster than the other, that it shall appear at length very much less, but still it will appear nearer. Here two images of the same object are seen under one view, and their apparent distances immediately compared; and here it is evident, that those distances have no necessary connection with the apparent magnitude. This experiment will be rendered still more convincing by the following means. If the glass be of some considerable breadth, let a small bit of paper, or the like object, be stuck upon it somewhere about the middle. Then let the inverted image be viewed through a short tube, or a hole at such a distance from the eye, that the frame of the glass or any circumambient objects may be concealed from the sight. When the eye has attentively considered this image, and fixed an idea of its apparent distance, let the glass be gently moved, while the eye accompanies the image, till the paper come also in sight; then as the image is actually formed nearer to the eye than the glass, so the paper will evidently strike the eye with the sense of its being more remote than the image. If the same trial be made with the upright image, the paper will appear nearest. This experiment succeeds best, when the eye is held as near to the glass, as it can bear to see the images distinctly.

13. LET US now consider Mr. Professor's arguments, in art. 138, against the received opinion. One of his reasons, that the different divergency of
rays

rays cannot be the means of suggesting to us the place, whence those rays come, is, that "it is a rational deduction from sense, which informs us, that rays diverge from the points of an object; which the majority of mankind are entirely ignorant of, and the ancient philosophers, who thought, that something like rays proceeded from the eye to the object, could distinguish distances as well as we." This is quite as good a reason for the rays of light being no cause of our seeing at all; because those, who know the rays come from the object, and those who imagined they issued from the eye, could see equally well. But certainly, as the rays of light make such an impression upon the eye, as to raise in us the ideas of the objects whence they proceed, though we have no immediate perception of those rays; so the change made in the eye, in consequence of the different divergency of the rays, may be a sufficient cause to suggest to us difference of distance, without our having any direct perception of that divergency.

14. ANOTHER argument is, that our sensible ideas of the places of the remote parts of a long walk or gallery, and of the clouds over head, and of all celestial bodies, are quite different from the rational ideas of the places, from whence the rays diverge. But these are objects placed beyond the utmost distance of distinct vision, and our not receiving information from this power in the eye of the distance of objects placed beyond its reach, is no reason against its producing that effect within the distances, at which it operates.

15. THE argument, drawn from the success of painters in imposing on the sight by projections in perspective, by the representation of lights and shadows, and other parts of their art, will then deserve to be considered, when a picture shall be produced not distinguishable from the original object at a di-

stance, in which the eye can see with perfect distinctness.

16. WHAT is said of objects seen through glasses by converging rays, is nothing to the purpose. For though we should admit this gentleman's opinion, that the appearance of their approach is to be ascribed to the augmentation of their visible magnitude, this can only be considered as one of the cases, in which the eye is subject to a deception from that cause.

17. HERE this gentleman has also objected against our forming a judgment of distance from the inclination of the axes of the eyes; because these angles are varied by turning the head side-ways, while we look at an object, though the distance remains the same. But as this is no proof, unless we saw things as perfectly by a side-view, as by a direct one; so in his remarks * on this article he corrects himself, and upon second thoughts, it seems, will not dispute, whether the feeling of the turn of our eyes in directing their axes successively from a remoter object to a nearer may not also contribute to correct our judgment of its distance.

18. As our author has acknowledged for his predecessor, in his opposition to the received doctrine, a writer remarkable for the singularity of his opinions, and has even adopted his arguments; I shall here occasionally observe, that that writer has in fact subscribed to the doctrine, he imagined himself to be opposing, in regard to the power of both eyes in this business. This is evident by comparing his words in the 17th section of his Essay towards a new Theory of Vision with the transcript, we set down above from James Gregory †. As Mr. Professor has exactly

* Art. 245.

† "Because the mind has by constant experience (quotidiana experientia, &c. Opt. Prom. J. Gregor.) found the different

actly followed the example of his original in assenting at last to the common opinion with regard to both the eyes, after having undertaken to oppose it; so possibly he might by his second thoughts have done the same in relation to one eye, had his author expressed himself somewhat more distinctly, where he has done so; for under the improper terms of straining the eye at Sect. 27, he has in reality acknowledged this consequence of the power, which fits the eye to each particular distance.

19. To apply this doctrine of apparent place to the assigning the apparent places of the images seen in optick instruments, it is necessary, that the apparent magnitude be assignable without regard to the apparent place, and for this end the antiquated doctrine contained in that ancient, but trifling piece of opticks ascribed to Euclide, that the apparent magnitude solely depends on, and is proportional to the angle, under which it is seen, is here at article 98 revived. Though Mr. Professor has delivered this maxim under the name of a definition; yet it is plain, he did not intend merely to impose an arbitrary sense upon the phrase apparent magnitude, but understood those words in their common acceptation for the idea of magnitude, which is excited in the eye upon the view of any object. Otherwise, what is said in the 104th and following articles concerning the apparent magnitude of objects seen by refracted or reflected rays, would be little more than tautology. Besides, he afterwards, at article 156, fol-

“rent sensations corresponding to the different dispositions of the
“eyes to be attended each with a different degree of distance
“in the object, there has grown an habitual or customary connection between those two sorts of ideas; so that the mind no
“sooner perceives the sensation arising from the different turn,
“it gives the eyes, in order to bring the pupils nearer or farther asunder, but it withal perceives the different idea of distance, which was wont to be connected with that sensation.”

lows

lows prop. 6th of the treatise abovementioned in assigning the diminution of the angle (without taking in any additional consideration) as the reason, why the distant parts of parallel lines seem to the eye not so wide asunder, as the nearer parts. He also repeats after prop. 10th and 11th of this treatise the like reason, why the remote parts of a walk or a floor appear to ascend gradually, and the cieling to descend; and after Aguilonius, whom the authority of Euclide's name led into the same error, that the upper parts of very high buildings seem to lean forward over the eye below, and that the distant parts of a line extended from the eye diminish to the sight*.

20. AFTER all this, in the 160th and following articles Mr. Professor relinquishes this notion of apparent magnitude, and acknowledges, that it depends in part on our judgment of the distance; that two rows of trees which are parallel, by standing upon an ascent, whereby the more remote parts appeared farther off, than they really were, the trees were thought to diverge; that animals and all small objects seen in vallies contiguous to large mountains appear extraordinary small, because we think them nearer to us, than they really are; that in like manner, when things are placed upon the top of a mountain, or upon a large building, and are viewed from below, we think them extraordinary small for the same reason; and in the last place, that the sun and moon in the horizon appear to us larger under the same angle, because they are judged farther off.

21. To reconcile these contradictions, in the remarks, art. 301, it is said, that there are two sorts of apparent magnitude. I hope, this gentleman does not mean, that objects have two apparent magnitudes at the same time: and if he only means,

* Optic. Lib. 4. prop. 2. & Consect.

that

that in some circumstances objects give us the idea of one apparent magnitude, and under other circumstances of another; he ought to have shewn, how it can be brought to pass, that the idea of apparent magnitude, which is unalterable, either by the power of the imagination or any circumstances whatever, that do not affect the picture of the object upon the retina, can be obliterated, and another depending upon a collateral circumstance take place in its room.

22. AS this gentleman is very large upon the appearance of the horizontal luminaries; and, by his referring at art. 333 of the remarks to Riccioli and a paper of Mr. Molyneux published in the Philosophical Transactions, Numb. 187, for the history of what had been done before him, seems desirous, the world should be duly apprized, how much he has contributed to the explanation of that appearance; these accounts being very defective, I shall here give a fuller history of this matter. Ptolemy in his *Almagest* Lib. 1. c. 3. ascribes this appearance to a refraction of the rays by vapours, which actually enlarge the angle, under which the luminaries appear; just as the angle is enlarged, by which an object is seen from under water. Thus he is understood by his commentator Theon*, who explains distinctly, how the dilatation of the angle in the object immersed in water is caused. This passage of Ptolemy we find copied by the two Arabian astronomers Alfraganus† and Geber‡.

23. BUT as soon as it was discovered, that there was no alteration in the angle, this cause was rejected. Accordingly we find, that Regiomontanus in his *Epitome* of the *Almagest* makes no mention of this cause assigned by Ptolemy, but refers to the

* Pag. 10.

† Elem. Astron. cap. 2.

‡ Astronom. Lib. 2. pag. 21.

optical writers *, who agree in another opinion: Alhazen an Arabian author somewhat later than Alfraganus and Geber, though he admits, that some such refraction may at particular times cause a more than ordinary appearance of this phenomenon, yet he denies this to be the constant cause, and that the appearance is not owing to any real augmentation of the visual angle, but to an optical deception. After he has endeavoured to shew in distinct propositions, that the ordinary refraction of the air would rather diminish than increase the visual angle at the horizon; then to account for the appearance in question, he repeats from a former part of his work, that the sight judges of the magnitude of visible objects by comparing the visual angles, under which they are seen, with their distances †, and that, if the eye does not form a true judgment of the distances of objects, it will not form a just opinion of their quantities. Next he observes, that the sight perceives the colour of the sky, but not its form; and whenever the sight perceives any colour extended in length and breadth without discerning its true form, it will conceive it, as flat, by resembling it to the usual superficies, which occur to the sight, such as walls, or the like, and this is the appearance of all convex and concave surfaces at distances very remote ‡: therefore that the sight apprehends the surface

* In ea re sensum decipi perspectivis conclamatum est. Lib. 1. Conclus. 1.

† Dico, quod in secundo tractatu hujus libri declaravimus, cum tractavimus de magnitudine: quod si visus comprehenderit magnitudines visibilium: comprehendit illas ex quantitibus angularum, quos respiciunt visibilia apud centrum visus & ex quantitibus remotiorum, & ex comparatione angularum ad remotiores. Optic. Lib. 8. n. 55. p. 280.

‡ Visus comprehendit colorem cœli, nec tamen certificat formam ejus nudo sensu. Et cum visus comprehenderit colorem aliquem in longitudine & latitudine: super hoc; quod comprehendit

face of the sky, as flat, and judges of the stars, as it would of ordinary visible objects extended upon a wide space; and when the sight views such objects dispersed over any extensive place, and sees them under equal angles, and perceives the quantities of their distances, then that, which is most remote, is esteemed the biggest *. Again, as the sight does not perceive the concavity of the sky, and considers the stars, as placed in it, it perceives equal stars to be unequal, when in different places. For it compares the angle, under which a star is seen near the horizon, to a remote distance, and the angle, under which the same is seen in a more elevated part of the sky, to a near distance, and estimates a star in or near the horizon to be greater than in the upper part of the sky; and therefore estimates the same star in different parts of the sky to be of a different magnitude †. And after having argued against the refraction of the air's being the cause of this appearance, because, though it conspired to this effect, it would be too small to

hendit figuram & formam: comprehendet ipsum planum: assimilabit enim ipsum aliquibus superficiebus assuetis, ut parieti & aliis. Et hoc modo comprehendit superficies convexas & concavas in remotione maximâ. Ibid.

* Visus ergo comprehendit superficiem cœli planam, & comprehendit stellas; sicut comprehendit visibilia assueta separata, quæ sunt in locis spatiosis. Et cum visus comprehenderit aliqua visibilia assueta in loco aliquo spatioso, & comprehenderit illa angulis æqualibus, & comprehenderit quantitates distantiarum visibilium: tunc illud, quod est remotius, comprehendetur majus. Ibid. p. 281.

† Visus ergo comprehendit superficiem cœli planam, nec sentit concavitatem ejus, & comprehendit stellas separatas in ipso. Comprehendit ergo stellas æquales, separatas inæquales: nam comparat angulum, quem respicit stella extrema, propinqua horizonti apud centrum visus, ad distantiam remotam, & comparat angulum quem respicit stella in medio cœli, & propinqua medio, remotiori propinquæ. Et similiter comprehendit stellam, quæ est in horizonte aut prope, majorem ea, quæ est in medio cœli aut prope. Comprehendit ergo eandem stellam & distantiam in diversis locis cœli, diversæ quantitatis. Ibid.

produce the appearance, he sums up the whole thus :

“ *Causa ergo, propter quam videntur distantiae stellarum in horizonte majores, quàm medio cœli aut prope : est illud : quòd sensus æstimat illas distare magis in horizonte, quàm in medio cœli. Et hoc, quòd visus comprehendit stellas in diversis locis cœli diversas in magnitudine : est error perpetuus : quia causa est perpetua : & est : quoniam visus comprehendit superficiem cœli planam, nec sentit concavitatem ejus & æqualitatem distantiae à visu.*” Ibid. pag. 282.

24. THIS cause of the appearance is so fully set forth by this writer, that he has been almost universally followed ever since. Vitellio assigns the same*, and receives therein the approbation of his commentator Kepler †; infomuch that, where he seems to join the refraction of the air as an additional cause, Kepler ascribes it to a typographical error ‖.

25. OUR countrymen John Peckham Archbishop of Canterbury and the famous Roger Bacon have very distinctly expressed the same. The former in a treatise entitled *Perspectiva Communis* first lays down this proposition, “ *Distantiam horizontis majorem apparere quam alterius cujuscunque partis hæmispherii.*” His reason is, “ *Ubi major magnitudo interjacere videtur, necesse est, ut etiam major distantia esse videatur. Sed inter horizontem & videntem tota terræ latitudo interjacere videtur. At inter videntem & punctum cœli verticale nihil interjacere videtur ‡.*” Then his 82d proposition is this, “ *Stellas in horizonte majores apparere, quam in alia parte cœli,*” which he proves from the former proposition, “ *Quia magis distare videntur stellæ in horizonte, quam in alia cœli parte, ac tum in ortu, tum in medio cœli sub æquali an-*

* *Optic. Lib. 10. n. 54.*

† *Paralipom. in Vitell. p. 134.*

‖ Ibid. p. 132.

‡ *Lib. 1. prop. 65.*

“ gulo

“gulo videntur; sequitur stellas in horizonte majores apparere quam alibi. Quia res ex æquali angulo ad majorem distantiam relata, major esse judicatur.” The latter, though more concise, is equally explicite. “Quod visus judicat cœlum quasi planæ figuræ extensæ super caput in orientem & occidentem, quando aspicit ad alterum illorum. Sed quod videtur prope caput propinquius videtur, & ideo stella quando est in medio cœli videtur esse propinquior, & ideo in horizonte videtur magis distare, sed quod magis videtur distare, videtur esse majus, postquam sub eodem angulo videtur.” *Perspectivæ Distinct.* 3. cap. 6.

26. THIS was the opinion of the cause of this appearance, while the Arabick learning prevailed, and the same has been generally adopted since. Besides Kepler before mentioned, who proposes again the same opinion in his excellent *Epitome of Astronomy*, Lib. 1. pag. 81. Christopher Rothmannus somewhat earlier, being mathematician to the famous Landgrave of Hesse-Cassel, and contemporary with Tycho Brahe, at page 109 of his *Discourse on the Comet in 1585* in direct terms approves of the explanation of this phænomenon given by Alhazen. And Cardan still earlier has very distinctly expressed the same opinion in the following words. “Astra omnia, dum oriuntur & occidunt, majora videntur, quam in cœli medio, quoniam terræ magnitudo intermedia facit, ut oculus ea plus distare existimet, & ob id esse majora: nam & turris ulna major judicatur ab oculo illius distantiam comprehendente, tametsi minorem angulum in oculo faciat ulna ipsa *.”

27. AGAIN, Martin Hortensius in his *Dissertation on the appearance of Mercury in the sun anno 1631*, at pag. 42 expresses himself to the same effect, “Quia cœlum longius a nobis putatur distare in

* De subtilitate. Lib. 3. ¶ 4.

“horizonte

“ horizonte ob longissimum terræ tractum inter-
 “ jectum, in medio autem cœli videtur vicinius,
 “ quia nihil ei interjicitur. Apparent quoque stellæ
 “ majores in horizonte quam in medio cœli; cum
 “ eorum quæ sub eodem angulo videntur, quæ
 “ longius putantur abesse, majora appareant, quæ
 “ propius, minora.”

28. A FEW years after, Des Cartes in his Diop-
 tricks cap. 6. p. 93. gives expressly the same reason;
 “ Hæc astra [sol & luna] circa meridianum in cœli
 “ vertice minora apparent, quàm cum sunt in ortu
 “ vel occasu, & occurrunt inter ipsa & oculos no-
 “ stros diversa objecta, quæ judicium de distantia
 “ melius informant. Et astronomi cum suis ma-
 “ chinis illa dimetientes satis experiuntur hoc, quod
 “ ita jam majora, jam minora appareant, non ex
 “ eo contingere, quòd modò sub majori, modo sub
 “ minori angulo videantur, sed ex eo quod longius
 “ distita judicentur.”

29. THESE are the words of Des Cartes, which
 Mr. Molyneux in the paper, to which our author
 refers his readers, has so strangely misrepresented.
 Nor has Mr. Hobbes fared any better under his
 hands. For though he erred in supposing, there
 was any necessary connection between the center of
 that arch, into which the sky appears, as formed,
 and the center of the earth; yet he accounts for the
 appearance truly from our being to sense out of the
 center of that arch *; nay has extended this so far,

as

* “ Causa quare sol, luna, & cæteræ stellæ majores apparent
 “ prope horizontem quàm ab horizonte remotiores, ex eo ori-
 “ tur, quòd cum oculus sit in superficie, non in centro terræ
 “ magis distat ab horizonte aspectabili, hoc est, ab horizonte
 “ cæruleæ illius superficie quam vocamus cœlum, quàm ab e-
 “ jusdem summo culmine. Etsi enim terra ad orbem stellæ cu-
 “ juscunque rationem magnitudinis habeat inconsiderabilem,
 “ magna tamen est comparata cum distantia, quæ apparet ab
 “ ipsa ad loca stellarum apparentia.” As this writer had very
 little mathematical skill, no wonder he should conceive the sub-
 ject

as to observe this deception to operate gradually from the very zenith to the horizon, and that if the apparent arch of the sky be divided into any number of equal parts, those parts in descending towards the horizon will gradually subtend a less and less angle*.

30. JAMES GREGORY subscribes to the common opinion, though he agrees with Alhazen, that an extraordinary refraction by clouds may at particular times increase the appearance. *Geom. Par. Univers.* p. 141.

31. FATHER Malebranche also in the first book of his *Recherche de la verité* printed in 1673, has explained this phenomenon almost in the expressions of Des Cartes, and twenty years after in defending it, he supposed like Mr. Hobbes the vault of the sky to appearance spread into what, he somewhat improperly calls a demi spheroidé applati†. And he caused to be printed in the *Journal des Sçavans*‡, an Attestation signed by M. le Marquis de l'Hôpital, M. Varignon, M. Sauveur, and M. L'Abbé de Catelan, signifying that his reasons were demonstra-

ject so indistinctly; but immediately after he gives a better reason for this difference in the apparent distance of the different parts of the sky. "Accidit oculo horizontem prospicienti distantiam ejus terram interpositam legendo æstimare, unde magis videtur distare quàm a cœlo summo. Majorem autem distantiam apparentem sub eodem angulo visorio necessario sequitur imago major." *Hobbes de Homine* cap. 3. p. 17. If it be surprizing, that this author should here express himself in part so ill, in part so justly, it may be remembered, that he has been suspected of receiving assistance, in what he has writ both in geometry and opticks from the manuscript papers of a person of more skill, Mr. Warner the publisher of Mr. Harriot's *Artis Analyticae Praxis*. See J. Wallisii *Elench. Geom. Hobbian.* p. 116 & Sethi Ward. *Exercitatio in Philosophiam Hobbianam* p. 356.

* *Hobbes de Homine*, cap. 7. p. 41.

† *Reponse du P. Malebranche à M. Regis à Paris* 1693.

‡ *Ann.* 1694, N^o. x.

tive and clearly deduced from the true principles of opticks.

32. I WONDER our author should represent Mr. Molyneux's paper, as containing the last thoughts, which had been advanced upon this subject, excepting the fancy in the Essay toward a new Theory of Vision, which is here so diffusely examined; when in the same Philosophical Transaction, in which Mr. Molyneux's paper was published, there follows a discourse of Dr. Wallis, which also is quoted in the New Theory of Vision, wherein the Doctor assigns the same cause as those authors, we have here named. And since Mr. Professor thought fit to examine in form so trifling an opinion, he should not, methinks, have put us off with a bare referring to Riccioli for the history of the subject. For he has made mention of no other than the two opinions, which we have already taken notice of, excepting that of Gassendus; whereas Baptista Porta has given us no less than three others *, and Scheiner has likewise suggested a particular thought upon the subject, that the appearance might in part arise from the contraction of the perpendicular diameter of the luminaries, or the perpendicular distance of stars near each other, whereby we may be led into a wrong judgment, and instead of considering these, as lessened, conceive the horizontal diameters of the luminaries, or the horizontal distance between two stars, as increased. Refract. Cœlest. cap. 28. pag. 46.

33. BUT indeed none of these obscure opinions deserve any particular examination; nor even that of Gassendus, though an author of greater fame, who would ascribe the appearance merely to a dilatation of the pupil, which he supposes to attend the view of celestial objects near the horizon †; for this is

* De Refract. L. 1. prop. 12.

† De apparente magnitudine solis humilis & sublimis Epist. 1. art. 5.

inconsistent with the principles of opticks, and the author of it is so totally ignorant in that science, as to assert, that a dilated pupil magnifies an object for the same reason, as a convex glass does *. His letters upon this subject contain likewise divers other absurdities; that the office of the crystalline humour is to erect the images of objects, which otherwise would appear inverted †; that no object is ever seen by both the eyes at once for this strange reason, that because an object viewed first with one eye alone, and then with the other, appears to cover different places of a distant wall; therefore if the axis of both eyes were together directed to the object, both those parts of the wall, if not the whole interval, ought to be hid ‡; nay, great part of those letters is taken up in reviving and justifying the ridiculous Epicurean scheme, that objects are seen by species continually flying off from them.

34. NOR do we find, that this opinion of Gassendus gained any credit with the skilful, however a writer or two of little note may have fruitlessly endeavoured to make sense of it. On the contrary Huygens in his treatise upon the Parhelia, translated by our author, has approved, and very clearly explained the received opinion §.

35. THIS

* De apparente magnitudine solis humilis & sublimis Epist. 2.
art. 13. pag. 39.

† Ibid. art. 4. pag. 18.

‡ Ibid. art. 17. pag. 52.

§ “ The cause of this fallacy in short is this, that we think the
“ sun or any thing else in the heavens to be remoter from us,
“ when it is near the horizon, than when it approaches towards
“ the vertex, because we imagine every thing in the air that ap-
“ pears near the vertex to be no farther from us than the clouds
“ that fly over our heads; whereas on the other hand we are used
“ to observe a large extent of land lying between us and the ob-
“ jects near the horizon, at the far end of which the convexity

35. THIS apparent greater distance of the sky near the horizon is so very obvious to the sense, that we find it assumed, as indubitable, upon all occasions. Aguilonius, though he does not consider the effect of this appearance upon the luminaries, or the distance of stars, has in the very page quoted by our author, in article 160, for another purpose this proposition, "*Cœlum prope horizontem longius a nobis distare videtur quam juxta verticem*". And Merfennus in his *Opticks*, pag. 495, says, "*Vifus non deprehendit quantum astra distant a nobis, & cœlum terræ in ambitu horizontis cohærere putat: res enim ut plurimum propinquoiores existimantur, quarum intermedium spatium non percipitur: hinc cœlum prope horizontem longius quam juxta verticem a nobis distare videtur.*"

36. BUT Mr. Hobbes is the first, who has expressly considered this vaulted appearance of the sky, as the real portion of a true circle, and Mr. Professor has followed him herein so far, as to improve upon his thought of dividing this arch into equal parts; proposing to find what portion of a circle this arch contains by observing the angle of the elevation of the middle point between the zenith and horizon.

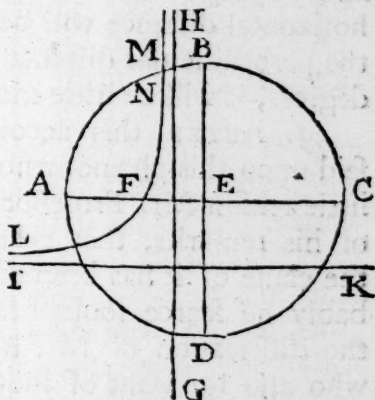
37. As this is the only addition, this gentleman has made, to what had been before advanced upon this subject, I am surprized to find the problem so very slightly passed over, since it is capable of a geometrical solution, which affords a better mechanical construction than the inartificial one, he ac-

" of the sky begins to appear; which therefore with the objects that appear in it is usually imagined to be much farther from us. Now when two objects of equal magnitudes appear under the same visual angle, we always judge that object to be larger which we think is remoter. And this is the true cause of the deception we have been speaking off." Art. 556.

* *Optic. L. 4. Prop. 4. Consect. 1.*

quiesces

quiesces in; and besides admits of a computation so concise, as to be preferable to either. If in the circle ABCD, whose center is E, two Diameters AC, BD be drawn perpendicular to each other, and in AE, EF be taken to AE, as the tangent of the angle observed to the radius, this angle being less than half a right the point F will fall between



A and E. Then EF being divided into two equal parts by the line GH parallel to BD, and ED also divided into two equal parts by the line IK parallel to AC, and to the asymptotes GH, IK the hyperbola LM being described through F cutting the quadrantal arch AB in N; the arch BN will be similar to half the apparent arch of the sky between the zenith and horizon. But the problem may also be solved by computation thus. Deduct the square of the tangent of the angle observed, and one third of that square each from the square of the radius. Then, as the radius is supposed unity, the arithmetical complement of the logarithm of the greater of these two numbers, half that complement, and the logarithm of the other number being added together give the logarithmick cosine of an angle, the cosine of the third part of which exceeds half the arithmetical complement of the greater of the two forementioned numbers by the sine of the angle, whose tangent is to the radius, as the apparent horizontal distance to the apparent perpendicular distance.

38. By this computation it will be easy to judge, how far the figure of the sky can be determined by observation upon this principle. As our author places the limits of the middle altitude within 18

and 30 degrees, if the altitude be 20 degrees, the horizontal distance will be scarce less than four times the perpendicular distance ; but if the altitude be 28 degrees, it will be little more than two and a half.

39. AFTER this account of what authors have said upon this phænomenon, it plainly appears, how little reason Mr. Professor has to assert in article 333 of his remarks, that ever since the time of Ptolemy the cause of it has been dubious and disputed. Probably he scarce looked farther into this matter than the dissertation of Mr. Molyneux, he here praises, who also for want of inquiry and mistaking the authors, he had seen, had thus represented it. But we find on the contrary, that from the time of Alhazen the opinion of the best writers had been, as it were, unanimous. Where this gentleman learnt, that the first astronomers were not aware, that the intervals of stars seem much greater near the horizon than the meridian, I cannot conjecture : for surely it is impossible to have lookt at the stars without perceiving it. Nor can I find, where Ptolemy has given that piece of advice, our author here ascribes to him. In the place, he has quoted, nothing is said upon the subject. But I suppose, he intended to refer to the second chapter of the ninth book of the *Almagest*, which is cited by Kepler on this occasion *, though nothing farther is there to be found than this short observation, that the difference in the apparent distance between stars in different elevations created a difficulty in judging of the exact time, wherein the planets are seen stationary.

40. IN this particular inquiry, besides shewing the true state of the subject, I had also a farther design, to set forth how well the principles concerning the apparent magnitude of objects had been established, and how generally approved, which this gentleman

* Paralipom. in Vitel. pag. 134.

has rejected for that crude conception concerning it, advanced in the first dawn of this science. But it will not be amiss to see farther, how distinctly optical writers have expressed themselves upon this head in general.

41. ALHAZEN has here delivered himself in very distinct terms, that though the apparent magnitude of objects had been by most ascribed to the magnitude of the visual angle only, yet that this opinion was undoubtedly false. He assigns this reason, that within moderate distances it is evident to the sense, that the object does not appear less to the eye by increasing the distance. That for instance the eye does not perceive a thing at two cubits distance, as if it were less, than at the distance of one cubit, nor even though it were removed to the distance of three or four*. He confirms this farther by observing, that if two diameters are drawn in a circle, and the circle

* Plures opinantur, quòd quantitas magnitudinis rei visæ non comprehenditur a visu, nisi ex quantitate anguli, qui sit apud centrum visus, quem continet superficies pyramidis radialis, cujus basis continet rem visam: & quòd visus comparat quantitates rerum visarum ad quantitates angulorum, qui fiunt a radiis, qui continent res visas apud centrum visus, & non sustentatur in comprehensione magnitudinis, nisi super angulos tantum. Et quidam illorum opinantur, quòd comprehensio magnitudinis non completur in comparatione ad angulos tantum, sed per considerationem remotionis rei visæ, & situs ejus cum comparatione ad angulos. Et veritas est, quòd non est possibile, ut sit comprehensio quantitatum rerum visarum a visu ex comparatione ad angulos, quos res visæ respiciunt apud centrum visus tantum. Quoniam eadem res visæ non diversatur in quantitate apud visum, quamvis remotiones ejus diversentur diversitate non magna. Quoniam quando res fuerit prope visum, & ipse comprehenderit quantitatem ejus: & postea fuerit elongata a visu non multum: non diminuetur ejus quantitas apud visum, quando ejus remotio fuerit mediocris. — non comprehendit visus rem visam in remotione duorum cubitorum, minorem, quàm in remotione unius cubiti. Et similiter si elongetur à visu per tres cubitos aut quatuor, non videbitur minor, quamvis anguli, qui fiunt apud visum, diversentur diversitate extranea. Opt. Lib. 2. n. 36.

so presented before the sight, that one diameter shall appear under a less angle than the other; or if the two sides of a square are offered to the sight in the same manner; yet, if the distance be moderate, the eye shall notwithstanding see one as a circle, the other as a square *. To shew how considerable a power the distance of the object has upon the idea, we receive from the eye of its magnitude, he afterwards proposes this observation; if a man should cover with his hand before his sight a great part of a distant wall, or of the sky, and then upon the removal of his hand observe the idea raised in his mind from that part of the wall or sky, which his hand before covered, he will perceive his imagination to be affected in no degree with the impression of his hand being of a like apparent magnitude with that part of the wall or sky, it covered †. This is so distinctly explained by this author, and so generally followed ‡, that Herigone has set it down as an optical axiom, "*Quæ sub eodem angulo videntur, quæ longius putantur abesse, majora apparent* ||."

42. THIS point therefore having been so generally received, I think Mr. Professor's fellow-writer might have spared himself the trouble of his long dissertation in support of this opinion §; which can only serve to persuade the world, that these gentlemen had but just come to the knowledge of so common a principle.

* Ibid.

† Ibid. n. 38.

‡ See Vitell. L. 4. prop. 27. Peckham Perspec. L. 1. prop. 64. Bacon Persp. Distinct. 3. c. 5.

|| Cursus Math. Tom. 5. pag. 15. This author conceived so little difficulty in the appearance of the horizontal moon, that he produces it as an indubitable proof of the axiom. "*Ut luna, quamvis in horizonte & medio cœli, sub eodem angulo cernatur, major tamen in horizonte quàm in medio cœli apparet; quod ejus distantia in horizonte major existimetur propter interjecta corpora.*"

§ Art. 171. of the Remarks.

43. WHEN Alhazen has expressed himself so distinctly upon this subject, as we have seen, it is a strange assertion of Tacquet, that every one of the ancient writers upon opticks supposed the visual angle alone to determine the apparent magnitude *. But this author can be of no account in this matter; since he had so little considered the operation of the eye, as to subscribe to the absurd assertion already mentioned of Gassendus, that the axes of both eyes are never directed to the same object †.

44. Aguilonius, though he adopts the same form of expression, yet by better considering the subject found himself obliged to acknowledge, that the eye does in fact estimate the magnitude of objects by comparing the visual angle with their distance: “Vim oculis natura concessit, qua simul rei distantiam percipiant, atque ex ejus collatione cum angulo pyramidis opticae, veram magnitudinem quam proximè dignoscant. Manifestè enimprehendimus errores omnes, qui in magnitudinis perceptionem irrepunt, ex distantiae ignoratione originem ducere ‡.” And he afterwards qualified the assertion, that those things, which appear under the same angle, are judged equal, and those, which appear under the greater angle, greater, by limiting it to the case, when the difference of distance is not taken cognizance of ||.

* Quæ sub æquali angulo videntur, apparent æqualia; quæ sub majori, majora; minora sub minori; & qualis est angulorum opticeorum, talis est & magnitudinum apparentium proportio. Id veteres optici ad unum omnes axiomatis loco habuere. Opt. Lib. 1. prop. 3.

† Ibid. prop. 2.

‡ Opt. Lib. 3. prop. 11.

|| PROP. X.

Majoribus spectata angulis majora, minora minoribus, æqualibus æqualia videntur.

In hac propositione subintelligendum esse, ut ratio disparis intervalli, quo magnitudines ab aspectu distant, penitus ignoretur. Opt. Lib. 4.

45. WHAT

45. WHAT Aguilonius has mentioned of the errors we fall into in relation to the apparent magnitude of objects being caused by a wrong judgment of their distance, Alhazen has particularly insisted on, when amongst the several causes of errors in vision, he sets down one to be great distance, and shews, that from thence will arise an error in our judging, both of the interval between distant bodies, and also of their magnitude †.

46. IT is this error in our judgment of distance, which causes parallel lines to seem converging, and the like deceptions, which in the optical treatise ascribed to Euclide, and by our author are referred to the mere decrease of the angle. Trees are sometimes planted so as to appear in one point of view, as parallel. But in these the angles subtending their distances continually diminish, as these distances are farther removed from the sight, though not so much, as when the trees are really parallel. If the angle only determined it, trees, which extended in two lines from the point of view, must be seen as parallel.

47. BUT now to sum up the result of this disquisition concerning the apparent magnitude of objects; as we have shewn the concurrent opinion of all the best writers to be, that it is connected with our ideas of their distance; so this gentleman has not only admitted as much in many objects viewed by the naked eye; but also in terrestrial objects seen through telescopes *, that the object does not strike the beholder with the appearance of its magnitude being increased as much as the angle, under which it is seen, because it is thought nearer. And this is certainly the true cause of the appearance. But I am very much surprized to see it affirmed, that, what is here said, expresses the true sense of his definition of

† Optic. Lib. 3. n. 52.

* Remarks, Art. 243.

apparent magnitude given in the 98th Article of his book, where it is asserted, that the greater visual angle not only causes a larger picture of the object on the bottom of the eye; but that this picture being larger or smaller causes a sensation of a greater or smaller visible extension.

48. IN short, what has perplexed Mr. Professor is this, that the apparent magnitude of very distant objects is neither determined by the magnitude of the angle only, under which they are seen, nor is in the exact proportion of that angle compared with their true distance, but is compounded also with a deception concerning that distance; inasmuch that if we had no idea of difference in the distance of objects, each would appear in magnitude proportional to the angle, under which it was seen; and if our apprehension of the distance was always just, our idea of their magnitude would be in all distances unvaried: but in proportion as we err in our conception of their distance, the greater angle suggests a greater magnitude. Our author not being apprized of this compound effect, has sometimes explained the apparent magnitude by the visual angle only, at other times by that angle compared with the distance, and by that means becomes thus inconsistent with himself.

49. HOWEVER besides this gentleman's inconsistency, the method he has taken to determine the place of images made by reflection or refraction simply from the magnitude of the angle, under which they are seen†, is certainly erroneous. For not to mention other examples to the contrary, when the inverted image made by reflection from a convex lens appears less than the erect image; though the inverted image seems nearest, yet it not only looks less to the sense, but also in reality subtends a smaller

† Art. 139.

angle, as may be easily proved by bringing one of the images over the other.

50. I BELIEVE, I need not apologize for the length of my remarks upon this point; because it relates to a principle, that extends itself throughout this whole work, and in which Mr. Professor triumphs as a great discovery in opticks unknown even to Sir Isaac Newton or Mr. Cotes†. But I shall now conclude this head with one short remark upon what, he has said concerning the inventors of the telescope. For, as he has promised us the historical, as well as the other parts of the subject; this account would have been less deficient, if besides the translation of Huygens's history of the invention, and the descant he has made upon Mr. Molyneux's claim in behalf of Roger Bacon, he had taken notice of the pretensions of another of our countrymen Leonard Digges Esquire. For if Roger Bacon had never compounded glasses together, it is certain, this gentleman had. For he expressly says in the 21st chapter of the first book of his *Pantometria*, published by his son Thomas Digges Esquire in 1571, "Marveylouse are the conclusions that may
" be perfourmed by glasses concave and convex of
" circulare and parabolicall fourmes, using for mul-
" tiplication of beames sometime the ayd of glasses
" transparent, whiche by fraction should unite or
" dissipate the images or figures presented by the
" reflection of other. By these kinde of glasses or
" rather frames of them, &c." Whoever reads the whole chapter, I think, can scarce doubt, but the author had seen in some measure the effects of viewing an object made by reflection from concave or convex surfaces through a lenticular glass.

51 HOWEVER, if Roger Bacon were quite ignorant of the telescope, yet I cannot agree, that the

† Remarks, Art. 197, 465, 494, 536.

use of glasses of small convexity for the relief of the sight decayed by age was so very imperfectly known by him, as this gentleman supposes*; for the more deficient or erroneous his theory is, the less probable is it, he should make such an assertion in terms so express (“hoc instrumentum est utile senibus”) if he had not seen the effect. The cursory manner, in which this is mentioned, persuades me, it was not an invention of his own; but I cannot conceive, how he should have said it so directly, if this use of glasses had not at that time begun to be in practice; especially in a chapter, which professes to exhibit examples of the doctrine before laid down. Mr. Professor’s opinion, that the fryar had here only in his thoughts a plano-convex glass with the flat side laid immediately upon the object, I cannot subscribe to. Not to examine into the criticism on the word “suppositi,” the consequence drawn from Roger Bacon’s considering in his reasoning only the refraction of one surface is, I think, certainly invalid: for by the same argument one might conclude, that telescopes are now most usually made with a plano-convex object-glass; because Dr. Smith has determined the relation of their lengths to their apertures, charges, and power of magnifying upon that supposition†. Certainly since Mr. Professor in so ample a treatise has thought fit to conclude these points from the case, where one refraction only takes place; though the object-glasses of those instruments in their common fabrick have two surfaces, which refract; why may we not suppose this old writer in that very imperfect state of the science to have taken the like liberty.

52. IN what relates to the philosophical theory of light, this gentleman has had the caution to set down Sir Isaac Newton’s discoveries for the most

* Remarks, Art. 88.

† B. 2. chap. 6, & 7.

part in his own words*; but has entirely omitted all, that he has established in his second book of Optics concerning the cause, why some part of the light is transmitted, and some part reflected at every pellucid substance. Now as this speculation is the strongest instance of the wondrous capacity of that great man in unravelling the most intricate and disguised operations of nature, and establishes at the same time such a series of properties belonging to light, as give us no small reason to hope, that by diligently following the clue, he has here put into our hands, still greater secrets of nature may one day be brought to view; Mr. Professor ought not to have passed over so important and complicated a subject by a bare hypothesis, that the passage of a ray of light at a surface, where it is reflected, is in a curve with one point only of contrary reflection at the very surface†. For if the light is inflected in its passage near the edges of bodies by the same principle, as it is reflected and refracted, and the alternate returns of easy reflection and easy transmission will give the light in its passage by those edges so many different bendings as to form three or even more visible fimbriæ to the shadows of bodies, why may not the same alternate dispositions at refracting surfaces produce in the light a more compound motion?

53. IN the remarks upon the cause of refraction and reflection, since Mr. Professor thought proper to explain the hypothesis of Leibnitz ‡ more at large than any other; it would not have been amiss for him to have ascribed it to its true author, Mons. Fermat ||; nor would it, I believe, have displeased a geometrical reader in so large a work, containing so

* B. 1. chap. 6, 7, 8.

† Article 191.

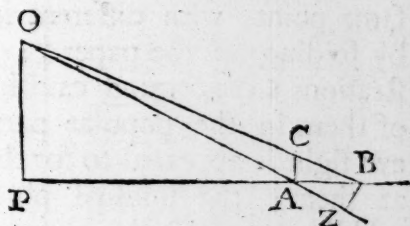
‡ Article 413.

|| See *Varia Oper. Math. Pet. Fermat*, pag. 156, &c.

many collections, to have seen Huygens's most elegant demonstration from his treatise of light *.

54. I SHALL now proceed to consider the 2d book of this treatise, in which we may discover perhaps one cause of Mr. Professor's falling into the above-mentioned mistakes. For here, I think, appears throughout that inexpertness in demonstration, which shews him to have been too little conversant in the writings of the best geometers. Of this we might have before taken notice of some examples. For instance, in article 157 it is said, that the apparent magnitude of a given line AB seen very obliquely at a given distance OA (meaning

the angle AOB, which it subtends at the eye in O) increases and decreases in proportion to the increase or decrease of



OP, the perpendicular distance of the eye from the line AB produced; provided that the distance AO be very large in comparison to AB. But though this assertion may be admitted, his proof is not to be justified. First, he says, if the ray BO cut a line AC perpendicular to AB in C, while the eye is raised or depressed in the perpendicular OP, the line AC will increase and decrease, as OP does: whereas this is false; for AC is to OP as BA to BP, which is not a given proportion, since neither BP nor AP are given, but AO. Again the angle AOB does not so properly vary in the same proportion with the line AC, as with a line let fall from A perpendicular on OB, or from B perpendicular on OA; and it is this perpendicular BZ, which is in a given ratio to OP, BZ

* *Traité de la Lumiere*, chap. 3. p. 40.

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being to OP as AB to AO. From this wrong turn given to the demonstration he was obliged to put an unnecessary restriction upon the proposition; for it is equally true, when the distance AO bears a very great proportion to AB, whether AB be seen with a great or small degree of obliquity.

55. BUT in the second book we find perpetual proofs of this unskilfulness in regard to geometrical demonstrations. Hence his complaint against the great Huygens's demonstrations for being tedious, intricate and embarrassing to the reader, by what he is pleased to call formal compositions and resolutions of ratios *; the inartificial method of marking the same points with different letters †; demonstrating by folding up the paper ‡; grounding his demonstrations || concerning caustics upon the description of them in the popular part of his treatise, where eye sight is appealed to for the light being condensed at them; the unusual phrases of proportionable, middle proportional; conjointly to signify every method of adding the terms of proportionals, and disjointly both for division, conversion, and taking the difference of the antecedents and of the consequents. To the same cause was owing the great inaccuracy, with which he expressed himself in relation to what, he calls the center of a lens. This likewise could be the only reason, which should induce him to take refuge in algebraical calculations **, in a case, whose demonstration is no more than this. XY being to TX as AP to PT, and XT to XY as TP to Pβ, by equality XT is to TX as AP to Pβ, and by composition T† to TX as Aβ to βP, or as Bβ × βA to Bβ × βP. Again TF being to F† as APq to Pβq, by conversion TF is to T† as APq to Aβ × βB. Therefore T† being to TX as Aβ × βB to Pβ × βB,

* Remarks, Art. 421, 475.

† Fig. 440 & 443.

‡ Art. 214.

|| Art. 329.

** Art. 339.

by

by equality TF is to TX as APq to $P\beta \times \beta B$. Consequently TX is greatest, when the rectangle under $P\beta B$ is greatest, that is, when it is equal to one fourth of the square of PB or AP, and then, since TF is to TX as APq to $P\beta \times \beta B$, TX will be equal to a fourth of TF, and XY equal to one fourth of FG.

56. OUR author seems himself conscious of this imperfection, when he takes advantage from his subject to express the lemma, whereon Sir Isaac Newton builds his demonstrations by prime and ultimate ratios in such terms, as he thinks, may be allowed in a physical subject, but in a demonstration purely mathematical have certainly no meaning. For, when it is said, that quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last, were he asked to explain, what that state is, which can be understood immediately to precede the last in any subject of geometrical demonstration, it were impossible to make sense of the question. Nay, so apprehensive is he of a defect in his geometrical reasoning, that he even refers to computation (a method, he has in another place * censured) as an additional confirmation †.

57. BUT besides such imperfections we find several direct errors. The fourth chapter has affixed to it a very pompous title, though it contains no more than two propositions, and those relating to the very

* Remarks, Art. 421.

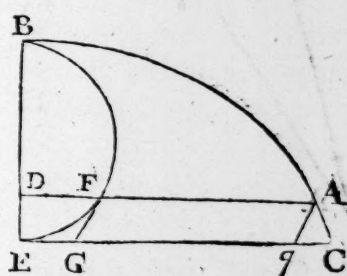
† The whole lemma runs thus. "Quantities and their proportions, which so approach to a state of equality as to become equal at last, may be taken for equal in a state immediately preceding the last; and also in a state somewhat remote from the last without sensible error in physical subjects: and the same may be said of figures, which continually approach to a state of similitude; especially if these errors, when computed, are found inconsiderable." Art. 204.

easiest cases. The second of which gives only an approximation except in the sphere. The first is scarce true in any kind of object besides a plane or curve line parallel to the refracting surface. It is here only proved, that the distances of any intermediate point of a streight line from the extremes are in the same proportion in the image, as in the original. But streight lines are never in geometry said to be similar, because they are proportionally divided. If this term is to be applied to the right line, it must be said without restriction, that all streight lines are similar. In every other object not parallel to the refracting surface, the proposition is directly false. If the object were the arch of a circle in any plane not parallel to the plane of refraction, its image, as here assigned, would be elliptical. It is also false in every plane object, not parallel to the refracting surface; and much more so in every solid object.

58. INDEED the demonstrations in the first chapter of the foci, besides the imperfection of the principle, upon which they are grounded, are on another account also inconclusive. As Dr. Barrow has searched with much more exactness than any, who had gone before him, into the place, where rays are collected by reflection or refraction, and where the images of objects formed by this means are seen; so his exact inquiry into the line, in which each ray passes, and in what degree they condense together in every circumstance drawing out his disquisition into some length, others have endeavoured to give a more concise, though less perfect idea of this subject. For this purpose, in particular, Dr. Gregory in his Elements of Opticks has by a method of demonstration similar to that of our author contented himself with shewing, whereabouts those rays would cross the axis of a reflecting or refracting spherical surface, which would enter the eye placed in that axis; and thus

thus far his demonstration may be admitted as just. But our author having used the same method to assign the place of the focus independent of the eye; his demonstrations are by no means so free from exception. For in the first part of his work *, he has always represented as necessary to constitute a focus, that the rays be in that place condensed together; but his form of demonstrating may be applied to cases, where there is no condensation of rays. For

example, if rays parallel to the base of the cycloide BAC generated by the revolution of the semicircle BFE upon the line EC be reflected, as the ray DA is reflected into Aq, as the point of incidence A approaches towards C, the lines Eq, EC continually



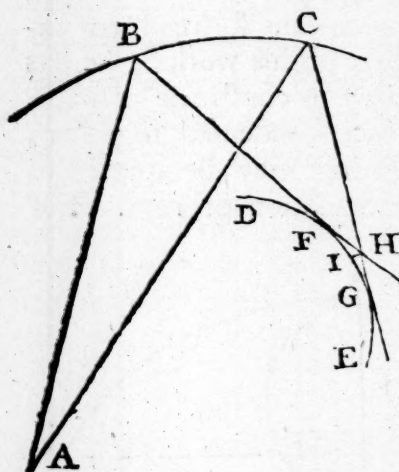
approach toward equality, and become equal, when the distance DE vanishes; yet here is no condensation of the rays on the axis at the point C; because the intervals between the points q appertaining to equidistant rays increase, while DA approaches EC. This is evident, because Aq is parallel to FG, the tangent to the semicircle BFE at the point F, where the rays intersect that semicircle, and Cq is equal to the excess of the arch EF above the tangent.

59. ARTICLE 468 contains a false conclusion from the preceding principles. For the density of the light, which is proportional to its heat or warmth, is to be estimated by a perpendicular section of the reflected rays, and to be determined by the aberration of the rays from the point of contact at the caustic after this manner.

60. If rays diverging from any point A are reflected by the surface BC into the caustic DE, all

* Book 1. chap. 2.

the rays in the pencil BAC will be spread through the part of the caustic FG, supposing F and G to



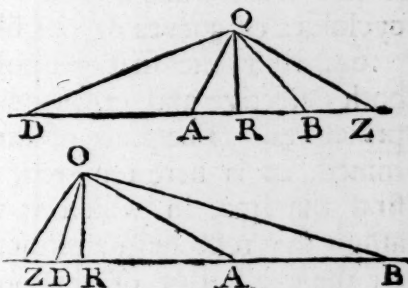
be the points, in which the reflected rays BH and CH touch the caustic. Here the density of the light upon the caustic must not be measured by the magnitude of FG, but by a perpendicular section of those rays, where they are most condensed. Thus if HI be drawn from H perpendicular to the curve, every ray of the pencil

BAC will pass through that line HI. This line then will nearly represent the magnitude of a perpendicular section of those rays at I; and therefore if two pencils proceeding from A are taken so that that the quantity of light, or, which is the same, that the angles made by the extrem rays may be equal in each, the densities of the light at the middle of the parts of the caustic formed by each pencil will be reciprocally as the line IH in each. But IH is ultimately the chord of $\frac{1}{2}$ the angle BHC to the radius GH or GI. The density then estimated by this means will be reciprocally as GI, or its double GF compounded with the reciprocal of the angle BHC.

61. HENCE it follows, that when to equal angles made by the incident rays at A unequal angles are made by the reflected rays at H, the density of the rays will not be justly estimated by the reciprocal of the part FG of the caustic included between the reflected or refracted rays. For instance, though in our author's case, at art. 465, when the rays reflected

reflected by a circle issue from a point in its circumference the density of the light in the superficial caustic, which is proportional to its heat, is, as this gentleman has computed, reciprocally as $bH \times HI$: yet in his 467th art. when the rays are parallel, this density is not in the simple ratio of BD directly and the triplicate of CD inversly, as he has determined; but in the duplicate ratio of DB directly and in the triplicate of CD inversly.

62. WHEREAS this gentleman professes to have rendered the doctrine of the rainbow more general, than it has been yet handled; all that he has added to what Dr. Halley had in the Philosophical Transactions Numb. 267. already done upon that subject, is the applying to the computation of the inverse problem a general theorem of Mr. John Bernoulli published in the Acta Eruditorum *, for finding from the tangent of an arch the tangent of any multiple of that arch. The lemma in article 508, from which this general theorem is deduced, might have been demonstrated thus. If ROA be one angle, ROB the other, and ROD their sum or difference. Draw OZ perpendicular to AO, then the angle ROZ is equal to the angle RAO, and therefore

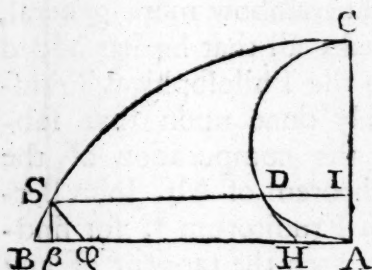


the angle ROB being equal to the angle AOD, that under RDO is equal to that under BOZ; whence as DZ to OZ so is OZ to BZ, but as OZ to ZR so is AZ to OZ, so that by equality, as DZ to RZ, so is AZ to BZ, and by division as DR to RZ, so is AB to BZ, or by permutation DR is to AB as RZ to BZ, or as $RZ \times AR$ to $BZ \times AR$, that is, as ORq

* Ann. 1722. Mens. Jul.

to $ORq - AR \times RB$, when AB is the sum of the given tangents and RD the tangent of the sum of the arches, but as ROq to $ROq + AR \times RB$, when AB is the difference between the given tangents, and RD the tangent of the difference of the arches.

63. THE proposition contained in article 602 is not universally true, but fails, whenever the curvature at the vertex of any one of the curves cannot be compared with that of the circle. For instance,



in the cycloide ABC , when any ray parallel to the base AB is reflected at S , $B\phi$, the aberration from B , is equal to the arch AD lessened by the tangent DH or HA , and $B\beta$ is equal to $AD - DI$; but

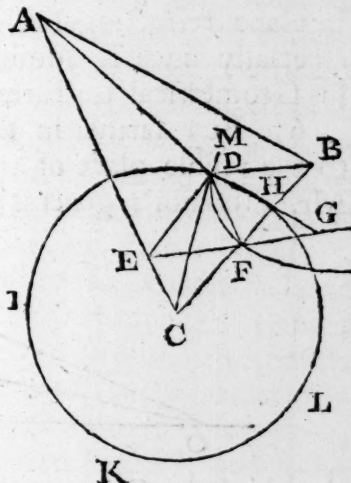
$AD - AH$ bears not ultimately a finite proportion to $AD - DI$. And hence the corollary in article 603 is not true, when one or more of the surfaces are cycloides or curves of the like curvature.

64. BUT the first proposition of this chapter is both defective and erroneous upon the author's own principles. The place of the image cannot be determined, as is here required, till it be shewn how to find the line, in which it is seen, that is, how to assign the reflected or refracted ray, which shall enter the eye. But of this our author has been absolutely silent. The problem is indeed in most cases too complex to admit of a simple and elegant solution; but this is no excuse, why in a treatise promised to be compleat, no notice should be taken at least of what had been done by others in this particular. Alhazen's problem to find the ray, which issuing from a given point should be reflected by a spherical surface to the eye in any given position, was thought worthy to exercise the invention of two

of

of the principal mathematicians of the last age. The hyperbola, by which Huygens solved the problem, especially as determined by Slufius in the Philosophical Transactions, Numb. 98. admits of a very concise demonstration. Suppose a spherical surface

IKL, whose center is C, and A to be a point, whence the ray proceeds, and B the eye. AC and BC being joined, and $AC \times EC$ likewise $BC \times CF$ made equal to the square of the semidiameter, and EF drawn and continued; if an equilateral hyperbola FM be described to the diameter EF, whose ordinates shall be parallel to



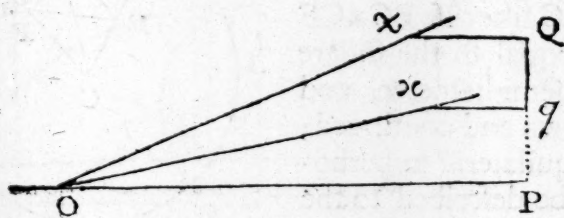
AB, the ray, which passing from A falls on the surface in the point D, where this hyperbola intersects the surface within the angle ACB, shall be reflected to the eye at B. Draw DHG parallel to AB, then by the hyperbola EG is to GD as GD to GF, so that the triangles EGD, DGF will be similar, and the angle DEG equal to the angle FDG. Again, $AC \times CE$ and $BC \times CF$ are equal; therefore AC is to CB as CF to CE, the triangles ACB, FCE are similar and the angle CEF equal to ABC or DHF. Consequently the angles DEG, FDH being also equal, the angles DEC and DFC will be equal. But if CD be drawn, AC is to CD as CD to CE; therefore the angle ADC is equal to DEC. For the same reason the angle BDC is equal to DFC; consequently the angles ADC, BDC are equal; and so the ray AD will be reflected to B.

65. If the opposite hyperbola be described, it will pass through the point C; and where it cuts the

spherical surface within the angle made by AC and BC continued will give the point in the concave surface, whence the ray from A will be reflected to B.

66. NEITHER ought Dr. Barrow's excellent solution given in his Optical Lectures * of the problem in plane refracting surfaces to have been omitted; especially since it admits of an improvement from his Geometrical Lectures †.

67. BUT farther in this proposition, what relates to the visible place of the image is inconsistent with Mr. Professor's general doctrine. For example, if



the object be Qq a part of the line QP , it is here determined, that Qx being drawn parallel to OP to meet the reflected or refracted ray, in which the point Q is seen, and qx drawn likewise parallel to OP , till it meet the like ray in x ; then the object Qq will be seen as extended from x to x ; but the general precept was, that the object appears in that place, where it would fill the angle, under which it is seen; whereas here the distance xx is greater than Qq .

68. WE have now gone through the two first books of this treatise, which are intended to comprehend as much of the subject, as is the proper object of mathematicks. In the next book, which promises the mechanical part, to moderate our expectations the author sets out with informing us, that he had himself no skill, in what relates to the figuring, grinding and polishing of glasses; and therefore has contented himself with giving us what collection, he could gather from others. Thus

* Lect. 5. §. 12.

† Lect. 6. §. 2.

there

there is no account of the common methods made use of; but only a particular artifice invented by Huygens, as better suited than the usual ways of working, to the formation of glasses for telescopes of an extraordinary length. But I shall take leave to observe upon this part of the work, that as our author professes to deliver the history and progress of the subjects, he has handled; in the descriptions of the astronomical instruments, which he has here inserted, he should not have subscribed to the vulgar mistake, that the method of graduation used in these instruments was the invention of Petrus Nonius *. For his method of division, which he explains at large in his very curious treatise de Crepusculis † printed at Lisbon in 1542, is widely different. It consists in describing within the same quadrant 45 concentric arches, dividing the outermost into 90 equal parts, the next within into 89, the next into 88, and so on till the innermost was divided into 46 only. By this means, in most observations, the plum line or index must cross one or other of those circles very near a point of division. Whence by computation the degrees and minutes of the arch might easily be counted ‡. This method of division is also described by Mr. William Barlowe in his book, entitled, The Navigator's Supply printed at London in 1597; and an actual example of it is to be seen in the

* Art. 861.

† Part. 2. prop. 3.

‡ This artifice Nonius likewise 20 years afterwards describes briefly in his excellent treatise De Arte atque Ratione Navigandi, where he would persuade himself, that it was not unknown to Ptolemy, saying, "Ita enim existimo Claudium Ptolemæum fecisse. Nam si maximam solis declinationem idcirco (ait) reperisse partium 23. m. 51. se. 20. quia ea proportio inventa fuisset totius circuli ad arcum inter tropicos quam 83 habent ad 11. Constat igitur aliquem quadrantem intra ambitum instrumenti descriptum, in ipsos 83 æquales partes distributum fuisse, quarum arcus inter tropicos 44 continebat. Neque enim tanta fuit illius instrumenti quo Ptolemæus utebatur magnitudo, ut in eo prima atque secunda minuta notari possent." Lib. 2. Cap. 6.

first figure of Tycho Brahe's *Astronomiæ Instauratæ Mechanica*, printed in the same year, But as this divides the degrees very unequally, and is also very difficult to execute with exactness, especially when the numbers, into which the arches are to be divided, are incomposite, of which there are no less than nine; though Tycho in his second instrument endeavoured to lessen the latter difficulty by the use of other numbers; yet to that instrument he also added diagonals, which he so much preferred, that at length he left Nonius's way quite off*, and used only diagonals in his mural Arch and the rest of his instruments.

69. THIS method of diagonals was first published by Thomas Digges Esquire, in an ingenious treatise of his entitled *Alæ seu Scalæ Mathematicæ*, printed at London in 1573, occasioned by the memorable new star, which had then lately appeared in Cassiopea, on which he made his observations by a fore-staff thus divided. But he acknowledges, it was not a discovery of his own, having been long practised in England; and, as he had been informed, was the invention of one Richard Chansler a most skilful artist†. Tycho, in that part of his *Progymnasmatæ*, which treats of the same star, takes notice of this passage of Mr. Digges, and affirms, when he studied at Lipsick, he used such an instrument divided after this manner, which was communicated to

* De Cometa Ann. 1577. pag. 461.

† —“ libere fateor illam partiendi radium in plurimas sensibiles partes rationem a me inventam non fuisse, sed diu hic in Anglia a plurimis peritissimis mathematicis usurpatam. Primus tamen qui ea divisionum ratione usus est, quemadmodum ex auditu accepi, quidam fuit nomine RICHARDUS CHANSLERUS, peritissimus & ingeniosissimus artifex mathematicus, cujus nomen eo libentius publicare decrevi, quòd jam e vita discesserit, neque monumentum suarum virtutum ullum publicum reliquit, præter instrumenta quædam summa arte fabricata, & dulcissimam suæ singularis peritiæ (in nonnullorum mathematicorum adhuc superstitum animis) memoriam.” Capitul. 9.

him from one Homelius, a famous mathematician of that place *. This was about 1565; for in a letter written to Rothman in 1587, he says, he was then 17 years of age †.

70. BUT Jacobus Curtius, Vice-Chancellor of the empire, improved at different times the division by concentric circles. At first, after describing within the limb of the quadrant a number of such circles, he extended the first to 91 degrees, the second to 92, the third to 93, and so on; and then divided each of these into 90 equal parts. This, when he was the Imperial minister at Rome, he communicated to Clavius, who published it there Anno 1586 in a treatise, where he describes an instrument for delineating of dials ‡; and Clavius himself proposes a variation in this method by describing 39 such concentric circles within the limb of the quadrant, continuing the second of these to 91 degrees, the third to 92, and so on, till the 39th extended to 128 degrees. Each of these circles he divided into 128 equal parts, which is performed by the simple method of bisection §. But a calculation is necessary, or at least a table ready computed, for knowing the degrees and minutes, which answer to the divisions of the several circles in both these methods; and Curtius made a second improvement, which required no such computation.

* “Ego certè multo ab hinc tempore, videlicet annis plus minus 28, cum Lipsiæ studiis incumberem, ejuscemodi partitionem radii in usu habebam, ex clarissimi mathematici Homelii officina, beneficio Bartholomei Schulteti, qui illi infervierat, mihi communicatam. Unde autem Homelius hanc hauserit, aut an ipsemet eam adinvenierit, apud me incertum est. Sit cujuscunque velit, ingeniosa certè & apprimè utilis est destributio, quam & ego postea arcualibus graduum subdivisionibus in quadrantibus, sextantibus & armillis non inconcinne aut infrugeserè applicui.” Pag. 671.

† Epist. Astronomic. pag. 62.

‡ Fabrica & usus Instrumenti ad Descriptionem Horologior. Cap. ult. p. 114.

§ Ibid. pag. 116.

71. THIS

71. THIS he sent about 1590 both to Clavius and Tycho, which the first published in his book of the Astrolabe, Anno 1593 *; and Tycho printed Curtius's letter with the account of this improvement, at the end of the abovementioned treatise, where he gives a description of his instruments. This method consists in setting off upon the first concentric arch within the outermost, the 60th part of such a portion of that arch as answered to 61 degrees, and from that division continuing on through the whole arch the intervals of single degrees. By this means every division in this arch is advanced one minute forwarder than in the first. At the beginning of the next arch he takes off the 60th part of 62 degrees, and from that point continues through the whole arch the intervals answering to single degrees; whereby each division in this arch is advanced two minutes beyond the degrees of the first. And thus he proceeds, till the degrees are divided into the whole number of minutes they contain †.

72. BUT

* Lib. 3. Canon. 1. Schol. pag. 566.

† This worthy gentleman Jacobus Curtius was the great patron of Tycho with the Emperor Rodolph the second, when Tycho had been so ill used by his countrymen. He was also a very ingenious person, as appears from these methods of division, as also from a scale of his invention for readily drawing the hour-lines, and a quadrant, which by the help of a table of sines and tangents is of excellent use in astronomy and practical geometry. Both these are described by Clavius in the 16th, and last chapters of his abovementioned book of dialling. Curtius likewise made an improvement in trigonometry, which he takes notice of in his letter to Tycho. This was occasioned thus. In 1588 one Nicolas Raymarus Ursus, of whom both Tycho and Rothman often complain as a plagiarist, published at Strasburgh a sort of rhapsody, which he called *Fundamentum Astronomicum*, where amongst many things good and bad, at fol. 16 and 17 he gives a scheme and directions how to resolve several cases in trigonometry by addition and subtraction; this Curtius informs Tycho, he had improved so as to include all cases. He does not there directly set down his method, but only hints, how it might be

72. BUT as this last improvement is very operose, a most excellent compendium of it has been since introduced. This was first communicated to the world by Peter Vernier a person of distinction in the Franche-Comté, in a very small tract, entitled *La Construction, l'Usage & les Proprietez du Quadrant nouveau de mathematique*, &c. printed at Brussels in 1631. In his dedication to the Infanta of Spain, Isabella Clara Eugenia governess of the Netherlands, having shewn its preference to what Nonius and Clavius had done in the affair he adds, " *Le mien ayant*

be done. Pitiscus at the beginning of the fifth book of his trigonometry in 1600 gives a method to this purpose; but Clavius had several years before in his abovementioned treatise of the *Astrolabe*, Lib. 1. lem. 53. performed it much better. This method may be briefly represented thus. The product arising from the multiplication of any two numbers is equal either to half the sum or to half the difference of the cosines of the sum and of the difference of the arches, to which the numbers proposed shall be the sines, multiplied by the radius; the sum of the cosines is to be taken, when the sum of the arches exceeds a quadrant, and the difference of the cosines, when that sum is less than a quadrant. If either or both the numbers exceed the radius, so as not to be found among the sines, such numbers must be divided by 10, 100, or 1000, &c. till they can be found in the table, and the produce multiplied accordingly. In like manner the quotient arising by the division of any number by another, will be equal to half the sum or half the difference under the condition abovementioned of the cosines of the sum and of the difference of two arches, one of which has for its sine the dividend, and the other for its co-secant the divisor, that sum or difference being divided by the radius. But if the dividend exceed the radius, it must be divided as before; and if the divisor be less than the radius, it must be multiplied in like manner, till it can be found among the secants. This directly follows from these two propositions; The first, that the rectangle under the sines of two arches is equal to the rectangle under the radius and half the sum or half the difference of the cosines of the sum and of the difference of the arches. The other is, that the sine and co-secant of the same arch are reciprocals. A report of this artifice seems to have put lord Napier upon the noble invention of logarithms. See Tycho Brahe's life by Gassendus p. 109. 165, as also Anthony Wood's *Athenæ Oxonienses*, Vol. I. col. 549.

" tous

“ tous ces avantages sur les autres ce n'est pas sans
 “ subject que je l'appelle nouveau et de mon inven-
 “ tion.” In the preface also he claims it as his own
 invention, and says, thereby a quadrant of three
 inches is rendered capable of determining minutes*.
 In his book he shews how to apply it to instruments
 of different dimensions. His contrivance is a move-
 able arch divided into equal parts, one less in num-
 ber than the divisions of the portion of the limb cor-
 responding to it †.

73. IN 1634 Joan. Baptista Morinus, one of the
 Royal Professors of mathematicks at Paris, printed a
 book entitled Longitudinum Cœlestium atque Ter-
 restrium Scientia. The way there delivered for find-
 ing the longitude had been publickly examined by
 commissioners appointed by Cardinal Richelieu. The
 author in graduating the instruments for making ob-
 servations to sufficient exactness commends two me-
 thods. The first, he says, was the invention of
 Joannes Ferrerius, a most industrious and accurate
 artist ‡. This is performed by circular diagonals,
 which

* “ —d'un instrument du tout admirable de mon invention,
 “ & qui n'a jamais esté veu, &c.—en un quart de cercle de
 “ trois poulces d'estendue, on demesle les minutes entieres du
 “ cercle quoy qu'il ne soit divisé qu'à l'ordinaire en nonante de-
 “ grées, à l'ayde toutefois d'une petite portion de cercle mobile
 “ contenant seulement quinze parties esgales; qui sont toutes les
 “ subdivisions necessaires au dict instrument.—

† In his first instrument after having supposed a quadrant to be
 divided into half degrees, he adds, “ La seconde partie de l'in-
 “ strument est une planche construite en la forme & figure d'un
 “ secteur de cercle, la circonference de laquelle comprendra juste-
 “ ment un angle de trente & un demy degrés; non toutefois
 “ formés ny descrits, mais bien divisé en trente parties esgales
 “ seulement.—” p. 10.

‡ “ Prima. Est accurata & geometrica divisio cujuslibet gra-
 “ dus in 60 minuta, quæ super Quadrante 2 pedum, imò unius
 “ pedis semidiametri commodè applicari potest; quam adinvenit
 “ D. Joannes Ferrerius instrumentorum mathematicorum soler-
 “ tissimus & accuratissimus fabrefactor, qui præsens aderat; ipsi-
 “ notissimus

which continued shall pass through the center of the quadrant, and which he demonstrates to answer to geometrical rigour *. But to save the trouble of drawing particular diagonals for every degree he advises one only to be affixed to the moveable index, which being divided into 60 equal parts, by its intersection with the streight lines proceeding from the center and distinguishing the degrees on the limb of the instrument, will give the overplus minutes †. The other method is Vernier's ‡, which Morinus particularly explains, and shews, if the radius of the quadrant be half a foot, one foot, two feet, or three feet, by a small arch divided into 30 equal parts answering to 31 of the divisions of the limb, along which it moves either as a sector, or as joined to the arm that carries the sights, angles may be measured true to one minute, 30 seconds, 15 seconds, or 6 seconds respectively. This he justly prefers to the other method ||, and describes it again in the continuation of his book, printed in 1636 §, and again

"notissimus Commiffariis, quibus inventionem suam pridem
"detexerat." Long. Scient. Par. 1. p. 17. This Ferrerius
must have been then very old, if he was the same, that is praised
for finding out a new method of drawing the hour-lines upon
dials by Clavius, in the preface to his abovementioned book
printed in 1586, in these words; "Inventor primus hujus ratio-
"nis, quæ præclarissima est, Hispanus quidam dicitur, nomine
"Joannes Ferrerius, homo in primis acutus, & in rebus invenien-
"dis admodum sagax.

* Long. Scient. Part 2. pag. 51, &c.

† "Hæc enim particula semper adhærens alhidadæ & secta à
"lineis quæ distinguunt gradus instrumenti, ostendet in sectione
"minuta gradibus addenda." Ibid. p. 52, 53.

‡ "Secunda est accurata etiam divisio 31 graduum five par-
"tium Quadrantis in 30 partes æquales; inventa & in lucem
"edita à Nobili viro Petro Vernerio in comitatu Burgundiæ
"castelli Dornansii Capitaneo; quâ cum Quadrante 2 pedum
"quarta pars unius minuti sensibiliter mensuratur." Ibid. Par. 1.
pag. 18.

|| Ibid. Par. 2. pag. 53.

§ Par. 6. pag. 187.

in

in a further continuation printed in 1639, where he recommends an azimuth quadrant of 5 feet radius, which by this artifice would measure angles to two seconds *; and lastly, in his answer to Longomontanus, printed in 1641, he mentions both these methods †.

74. NOTWITHSTANDING all this, in 1643 one Benedictus Hedraeus a Swede, published at Leyden a small treatise, entitled *Nova & Accurata Astrolabii geometrici Structura*, &c. where in the preface he takes occasion to describe circular diagonals without mentioning Ferrerius, and assumes to himself Vernier's method. This in his book he applies to a Theodolite and Protractor, by dividing such a part of a moveable circle as answers to 61 degrees of the limb of the instrument, into 60 equal parts ‡; he shews also how this moveable circle may be otherwise divided §, and likewise describes a large astronomical quadrant, which by a small moveable arch corresponding to 5 degrees 5 minutes of the limb of the instrument, each degree of which limb being divided into 12 equal parts, would measure angles to 5 seconds ||, or even half that.

75. THIS method is also succinctly explained by Tacquet in his *Opera Mathematica* ** printed in 1669, where he would ascribe the original of it to a proposition of Guido Ubaldi in his book of *Astronomical Problems* †† published after the author's

* Par. 9. Cap. 8.

† Coronis *Astronomiæ*, p. 14.

‡ "Limbo sic diviso, numerentur ab una linearum, quas vocant fiduciæ, ut in delineatione AB, in quam partem placuerit, 61 gr. ab A usque in C, & spatium illud in circulo mobili, quod hisce 61 gr. respondet, dividatur in 60 partes æquales." Par. 1. Memb. 1. cap. 2.

§ Ibid. cap. 6.

|| Par. ult. Memb. 1. cap. 2 & 3.

** *Geom. Pract. lib. 1. cap. 4. pag. 23.*

†† Lib. 1. Prob. 1.

death in 1609. But this was delivered very distinctly by Clavius in 1586 *, as the invention of Fabricio Mordente, the Emperor Rodolph's mathematician, who discovered it long before, as appears from a treatise of his brother printed at Antwerp in 1584, entitled *Il Compasso del Signor Fabritio Mordente : con altri istromenti mathematici ritrovati da Gasparo suo fratello*. Though this can at most be considered only as a very faint rudiment of these methods, being no more than a proposal to find the number of minutes, seconds, &c. of any arch less than a degree, by multiplying this arch 60 times, whereby the number of degrees, it measures, will express the number of minutes in the original arch; and if the multiple exceed an exact number of degrees, the excess multiplied in like manner will exhibit the seconds, whereby the arch given exceeds an exact number of minutes.

76. IN 1673 the famous Hevelius published the former part of his *Machina Cœlestis*, where he adopts this method with due applause; being the first that reduced it to practice in large astronomical instruments. He describes it from Hedræus, and appears not to have known, that it had been mentioned by other writers.

77. IN 1674, one Gerard à Gutschoven, Professor of the Mathematicks and Anatomy in the University of Louvain in a very small piece entitled *Usus Quadrantis Geometrici* printed at Brussels gives this method without taking notice of any preceding author. He applies it in the form of a sector † to a

* *Fabrica & Us. Instrum. &c. cap. ult. pag. 120.*

† “*Cursor est sector circuli mobilis, cujus partes sunt arcus minutorum, arcus nempe in 30 partes æquales divisus, ipsos Quadrantis gradus lambens.*” in præf.

small quadrant, whereby minutes may be measured*. This quadrant, which he so much boasts of, as not the production of his closet, but as brought into use in the fields and woods, has the rectilinear sides of the quadrant projected into two circular arches to make room for the sinical division in the middle †, which had been described by others ‡.

78. LASTLY, it is this method of Vernier, that Mr. Professor Smith has also described, which is indeed preferable to any form of diagonals, though Dr. Hook in opposition to Hevelius labours to prove the contrary §. The doctor has indeed removed the objection against the absolute exactness of rectilinear diagonals, when divided by equidistant concentric arches, by shewing how to divide them more truly by the help of a table of tangents ||. But as his ar-

* “ *Quadrans totus divisus est in 90 gradus ; Cursor autem in 30 partes æquales qui comprehendit accurate 31 gradus quadrantis. Quisque Cursoris gradus singulos Quadrantis gradus excedit unâ trigesimâ gradus, id est, duobus minutis primis, unde fit ut sequenti methodo non gradus tantum, sed etiam singula minuta prima à Regula fiduciæ abscissa numerare valeamus.* ” — Prob. 1.

† “ *Usui & praxi maximè appropriatum [instrumentum] mihi excogitavi non in Musæo vel Cathedra, sed in ipsis agris & filvis, in ipsa manuali operatione.* ” In præf. “ *And afterwards Habes Scalam altimetram ad arcum reductam variis usibus destinatam :—Tandem planum opplevimus lineis cancellatis, magnum in triangulis solvendis usum habentibus ; & hæc etiam de penû meo deprompta sunt.* ”

‡ The Arabians, who introduced the use of sines into trigonometry, seem to have been the authors of this device ; see Catalog. MSS. Angl. p. 461. N^o. 3309-6. But Petrus Apianus in his *Introductio Geographica*, printed at Ingolstadt in 1533, has given a figure of it with this inscription on the limb, “ *Quadrans universalis a Petro Apiano jam recens inventus ;* ” premising this character of it : “ *Quadrans novus quem universalem seu generalem libuit appellare, eò quod quicquid in primo mobili quæri, excogitari, aut proponi potest per hunc licet invenire.* ”

§ See his *Animadversiones on Hevelius's Mach. Cœlest.*

|| Ibid. pag. 22. and 23.

guments for their preference are very weak ; so he in vain would alledge the authority of Tycho on his side : for it does not appear from Tycho's works, that this compendium was at all known to him. The citations * thence given by the Doctor, refer only to that little improvement upon Nonius mentioned above.

79. THE truth is, neither of these methods are of any use unless executed with exactness ; and one great excellence of that of Vernier is its readily discovering the least error committed in the divisions. For which reason none but an able artist dare undertake it. But it is now performed very accurately in various instruments by our most skilful workmen, and its superior usefulness is sufficiently established by its having a place in the mural arch of our Royal Astronomer † ; whom the same ardor still supports, which above 60 years ago carried him to St. Helena to settle the southern constellations ; so that we may expect from his great abilities, and unwearied diligence, a series of observations upon the Moon's motion, that will far exceed, whatever has been produced since the very beginning of astronomy.

80. BUT to return to our author, whose fourth and last book promises a compleat history of the discoveries, that had been made in the heavens by the telescope, selected from a great variety of books, memoirs and observations of the best astronomers ; which books are these, Galileo's *Nuncius Sydereus* the first on the subject, the *Systema Saturnium* of Huygens, Du Hamel's History of the Royal Academy of Sciences at Paris, the *Memoirs* of that Academy, our *Philosophical Transactions*, and a late treatise of Bianchini, entitled, *Hesperii & Phosphori nova Phænomena*. But as in relation to the late wonderful discovery of an apparent motion in the fixt stars, which is occasioned by the gradual

* Ibid. pag. 3, & 4.

† Dr. Halley.

progreſs of light, wherein the worthy Savilian Profeſſor of Aſtronomy at Oxford, Mr. Bradley *, has ſhewn equal diligence and ſkill in obſervation in detecting the circumſtances of the appearance, as ſagacity in ſearching out the cauſe; ſince our author has only dilated upon what Mr. Bradley has himſelf writ, I ſhall conclude my remarks on this treatiſe with a ſhort account of an improvement communicated in 1729, as ſoon as Mr. Bradley's diſcovery was known, by the late great geometer Dr. Taylor to ſome of his friends; whereby the particulars of this appearance may be exhibited according to the exact theory of the earth's motion.

81. LET ABC be the orbit of the earth, in which it moves from B to C and A round the ſun in D. Then the earth being in C, if the tangent CE be drawn, and upon the tranſverſe axis AB as a diameter the circle AEBF be deſcribed, ED being drawn and continued to G, DE is perpendicular to the tangent EC; and therefore the velocity of the earth in B is to the velocity in C reciprocally as DB to DE; that is, directly as AD to DG, ſince the rectangles under ADB and EDG are equal.

Now CH being drawn from the earth in C toward

* Now Dr. Bradley, and the Royal Aſtronomer at Greenwich, as well as Profeſſor at Oxford.

the true place of any star, take CI to AD as the velocity of light to the velocity of the earth in B. Through I draw KIL in a situation perpendicular to AB, and making KI equal to AD and IL equal to DB, upon the diameter KL describe the circle KMLN parallel to the plane ACBF. Lastly, make the angle under KIO equal to that under ADG, which is nearly the arithmetical mean between the mean and true distance of the earth from the aphelion, and draw CO. Now KL being in a situation perpendicular to AB and the angles under KIO, ADG equal, OI will be equal to DG, and be situated perpendicularly to GE, and therefore is parallel to CE or to the direction of the earth's motion in C; and is to KI as the velocity of the earth in C to its velocity in B. Consequently since KI or AD is to CI as the velocity of the earth in B to the velocity of light, by equality OI is to IC as the velocity of the earth's motion in C to the velocity of light. Therefore the star will be seen from the earth at C in the direction CO. Hence it follows, that every fixed star is viewed from the earth as moving in a circle parallel to the plane of the ecliptic, the diameter of that circle which is parallel to the lesser axis of the earth's orbit being divided by the true place of the star in the same proportion, as the greater axis of the earth's orbit is divided by the sun.

R E M A R K S

O N

Dr. *JURIN*'s E S S A Y

U P O N

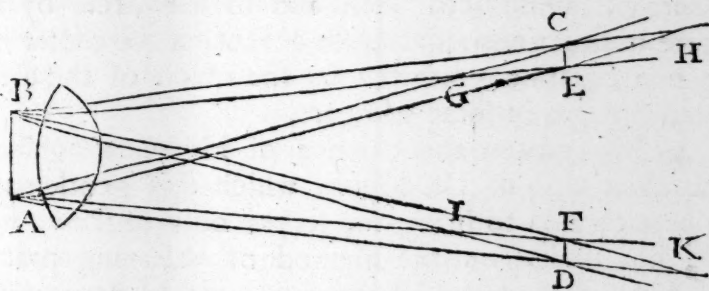
Distinct and Indistinct V I S I O N.

1. **T**O these animadversions upon Mr. Professor Smith's compleat System of Opticks I shall add some observations upon the Essay subjoined by his friend and fellow-writer Dr. Jurin. This discourse consists of four general heads. The first relates to the difference in the appearance of an object when seen distinctly, and when confusedly; the second to that faculty in the eye, whereby it accommodates itself to the different distances of objects; the third to a particular phænomenon in indistinct vision, which is the multiplication of the object; and the fourth is taken up in considering the dazzling, to which the eye is subject by the immediate contrast of light and darkness.

2. AS it appears, that this gentleman learnt the principles of opticks from his friend's book; so in his first head he has expatiated on the simple case of a uniform white object upon a black ground seen indistinctly more in detail, than one can suppose, a
writer

writer would have done, to whom the subject was not new : And after all he has treated of it but imperfectly, proceeding on a supposition, that the light is uniformly spread through his circles of dissipation; whereas he confesses at art. 220. that the density of the light is different in different parts of these circles. Nay, he had so ill fixt the subject in his thoughts, that when he was put to such hard shifts how to account, that the astronomers without the assistance of telescopic sights should observe the diameter of the moon nearer to the truth, than he could have expected, he had certainly forgot, that the magnitude of his circle of dissipation depends upon the breadth of the pupil; for as those observations were made either through a small hole, or slit much narrower than the pupil, the dilatation of the moon's image from its indistinctness must have been proportionably lessened.

3. WHEN he comes to consider at art. 90. an object but a little more complex, a black circular line seen upon a white ground, or two black parallel lines viewed in like manner, he has much mistook. It is a fact, that such a circle at one distance from the eye will appear to have a black spot in the middle, and the middle space between two parallel lines near together in the like situation will become dark; but



this is owing to a very different cause from what he has assigned: For if AB be the diameter of the pupil, and CE, FD the distinct images of the transverse

sections of the circumference of the circle, or of the parallels; and from the extremities of the pupil to the extremities of these images the rays AC, AE, AF, AD, and BC, BE, BF, BD be drawn, the spaces GCHE and IFKD will be deprived of light; but all without these spaces will be so much illuminated, that the axis of the eye can no where be deprived of light sufficiently to produce so distinguishable a degree of darkness. Neither is the appearance truly described by our author, for the middle darkness in the parallel lines, and the central spot in the circle do not extend into a simple penumbra gradually vanishing, but that shade is composed of a number of different images, and appears uniform, only when those images become so numerous, as not to be easily distinguished. And the true cause of this appearance is the multiplication of the image found in objects seen indistinctly; which I apprehend to arise from some corrugation, or inequality of surface, to which that part in the eye, which is changed for the different distances of objects, is subject in its extreme tension either way, whereby the image of an object looked at out of the limits of distinct vision is multiplied. From this multiplicity of images two parallels, as each is multiplied, at first are seen as four lines, or even more, but at length the two innermost unite into one; and in the circle by a more multifarious appearance a spot in the center is in one situation produced by the union of the different images in some one part.

4. MOREOVER the defence of Hevelius against the objections of Dr. Hook, which this gentleman is here pleased to bring in, serves only to shew, he did not understand the method of observing practised by that great astronomer. It is very well known, that Dr. Hook passed too severe a censure upon Hevelius's instruments, and his method of observation. But this mere imagination of an extraordinary

ordinary length of sight, which Hevelius might acquire from age and long exercise is a very trifling defence. But the truth is, his method of observation required no such length of sight. This might be justly objected to the method of observing practised before Tycho Brahe; but that noble restorer of astronomy invented a form of sights continued in use by Hevelius not chargeable with this inconvenience. The sight next the eye is a plane furnished with two parallel chinks, both of which, not one only, as this gentleman * has represented, are made use of in every observation. The other sight is either a plane likewise equal in breadth to the interval between the chinks, or a cylinder of that thickness. Now when these sights were pointed towards any star, a little before they came into the exact direction of the star, the light of the star would shine through one of the chinks, but be intercepted from the other by the interposition of the remoter sight, and when the sights had passed the direction of the star, the chink before obscured would now receive the light of the star, and the other chink be covered from it. But in the intermediate situation, which is the very direction of the star, the light of the star would pass through both chinks. Therefore it was the business of the observer to bring the sights into such a situation, wherein by looking through each of the chinks, one immediately after the other, his eye would receive the light of the star through both. This method of observation therefore is little influenced by the larger or shorter sight of the observer, but its accuracy depends upon the proportion of the breadth of the chinks to the distance between the sights: and those chinks were so framed, that by a screw or some other like contrivance they might be equally dilated, or contracted together. In this manner of observing Dr. Hook's assertion is evi-

* Essay upon distinct and indistinct Vision, art. 183.

dently

dently false, that a lesser angle cannot be distinguished better by a long radius than by a short one *.

5. IN the second part of this essay, after informing us, that he had deceived himself by certain trials to favour the singular opinion of Mons. de la Hire, that the eye was not possessed of the faculty, he now admits of, he tells us, that Dr. Porterfield in his dissertation lately published by experiments better contrived has caused him to change his mind, and subscribe to the common opinion.

6. IN relation to the new scheme, he advances, concerning this change in the eye; before it is any further considered, I think, we must ask the anatomists, whether a muscle strong enough to keep the eye tense can be invisible.

7. IN his third head this gentleman has had the hardiness to advance beyond the instructions of his master; and has adventured to expatiate upon the discoveries of Sir Isaac Newton, concerning the fits of easy reflection and transmission of light, where Mr. Professor has chose to be very concise. And as our compleat system of opticks is upon this argument so very deficient, that these alternate fits of reflection and transmission of light are not so much as mentioned by name, where he has undertaken to exhibit Sir Isaac Newton's discoveries in optics †; before I come to consider this writer's speculations upon this point, I shall briefly give some account of this matter.

8. It is well known, that when light falls upon any the most transparent substance, a part only enters the substance, some part being reflected at its superficies. Again, when the light, which passes into the substance, is arrived at its farther superficies, the whole is not transmitted through that superficies, but there also a part is reflected back.

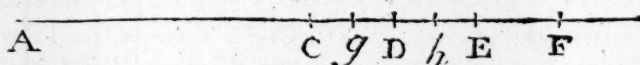
* Animadversions on Machina Cœlestis, pag. 7, & 8.

† Book 1. chap. 6, 7, 8.

When any colourless transparent substance is exposed to the open light of the day; what is reflected from its first superficies is always white, and so is the light returned from its farther surface, unless the substance be very thin; for in that case the light is coloured; and the colour differs according to the particular thickness of the substance. If such a thin transparent plate be unequally thick, the light reflected from its farther surface will be variegated with different colours. Such is the appearance seen in a bubble blown up with water rendered tenacious by the dissolution of a little soap. When this bubble is placed under a glass to protect it from being disturbed by the agitation of the air, the water, which composes it, will gradually and regularly descend from the upper to the lower part, rendering the bubble continually thinner and thinner till it breaks; the upper part also by this means will be always thinner than the lower. This bubble, when grown very thin, will be overspread with a great variety of colours, which Sir Isaac Newton has very particularly described. Likewise, if the object-glass of a very long telescope be laid upon a glass, that is flat, so as to touch it in one point, the small convexity of the telescope glass will leave between the two a thin plate of air of a considerable breadth perforated in the middle, where the glasses touch, and from thence gradually thickening. Here, where the glasses touch, and for some little distance round, the light is so freely transmitted, that no reflection is seen, but this dark spot will be encompassed round with a great variety of colours of the same kind with those seen upon the water-bubble. When the plate of air begins to be a little thick, these colours become of a very compound kind, and soon after end in perfect whiteness. In like manner the light reflected from the bubble of water at first, while its coat is thick, is uniformly white; and on the contrary, when it becomes extremely thin, a dark spot

spot appears on the top by the reflective power being in that place nearly quite lost.

9. THE cause of these phænomena Sir Isaac Newton found to be, as follows. The rays of light in their motion through the air, or any other transparent medium, are not alike disposed in every part of their passage in regard to reflection and refraction; in so much that in some places, if they meet with another transparent substance, they shall enter it freely, suffering only a refraction at the surface upon its transmission through into the transparent substance, but in other places the same ray shall be disposed to be reflected back from any such substance in its way. These alternate dispositions are found to return at equal intervals after this manner. Suppose AB to represent the path of a ray of light. Then if at the point C the ray be



disposed to enter freely any transparent substance interposed in its way, but at D be disposed to be reflected, from the surface of such a substance; if DE be taken equal to CD, at E it shall again be in a disposition freely to be transmitted through the superficies of such a surface, and if EF be taken of the same length with the former intervals, the ray at F shall be again in a disposition to be reflected. And thus will these two dispositions change and return at equal intervals throughout the whole progress of the ray within the same medium. But these alternate dispositions are not to be understood, as confined to these precise points; but if these dispositions are supposed to operate strongest at these points, they must be considered, as decreasing gradually from them both ways. Sir Isaac Newton divides these intervals into two equal parts, as here CD is divided at *g*, and DE at *h*; and considers the whole fit of reflection to be extended between *g* and *h*, but strongest at D, and from

from thence gradually weakening toward g and h , yet with this caution that it should not be conceived as precisely limited at these points g and h , but to decay indefinitely.

10. BUT farther, as the rays of light differ one from another in colour, and refrangibility, insomuch that there are rays of all degrees of refrangibility indefinitely from the least to the greatest; so the lengths of these fits of easy reflection and transmission are as much varied, and every degree of refrangibility is accompanied with a peculiar length of these fits, the least refrangible rays having their fits the longest, and the most refrangible rays the shortest within the same medium.

11. AGAIN in different mediums, and according to the different angle, under which the ray enters any medium, these fits are in the same rays different.

12. THE inequality in the length of these fits belonging to rays of different refrangibility and colour, is very great within the same medium, the intervals between the middle points of two nearest fits either of reflection or refraction in the most refrangible rays being to that length in the least refrangible as 63 to 100; whence it follows, that when the medium through which the light passes, is so thin a plate, that its whole depth contains but a small number of these fits in any one colour, the light of some colours may at its farther surface be in a fit of easy reflection, and the light of other colours in a fit of easy transmission, in which cases the light reflected will be coloured. Sir Isaac Newton exhibits for an instance of this the thickness of a plate, wherein the green light shall at the farther surface be in its third fit of easy reflection; there the yellow, and great part of the blue will in some degree be in a fit of easy reflection also; but no other part of the light: therefore the reflected light will be pure green compounded only with some yellow and blue.

13. HOWEVER to produce this coloured appearance the plate must be very thin; for these intervals in every one of the colours are very small; in the least refrangible rays, when the light falls perpendicularly upon a plate of air, the interval between the middle of two successive fits of easy reflection, that is, DF in the preceding figure, is not the 78000th part of an inch, and in the most refrangible rays scarce the 125000th part. In water these fits are about $\frac{3}{4}$ only of this length, and in glass not quite $\frac{2}{3}$. If the plate of air were but the 800th part of an inch, whereby according to computation from Sir Isaac Newton's numbers, the least refrangible rays would arrive at the farther surface in the 99th fit of reflection, a ray at the confine of the red and orange will have 107 fits of reflection within the plate, a ray at the confine of orange and yellow will have 111 fits of reflection, a ray at the confine of yellow and green 119 of those fits, a ray at the confine of green and blue 129 fits, a ray at the confine of blue and the indico 139 fits, a ray at the confine of the indico and violet 145 fits, and lastly the most refrangible rays 156 of these fits. Hence many species of the rays of each denomination of colour will be found in fits of easy reflection at the farther surface of such a plate; all which together can compose nothing short of perfect whiteness in the reflected light, and consequently the light transmitted will be equally free from any tinge of colour. Nay so soon does the light lose all diversity of colour, that Sir Isaac Newton found it in his observations to become white, as soon as it had more than 8 or 9 fits in passing through a thin plate*.

14. ANOTHER circumstance is here still to be considered. While the transparent plates in Sir Isaac Newton's experiments were so thin as to exhibit rings of colours; the rings, where the plate was thinnest,

* Optics B. 2. part 1. observ. 12.

reflected the greater quantity of light. The reason of this is evident from the method he has laid down for finding the colour reflected in each case, whereby it appears, that in the most luminous part of the rings, where the plate was thinnest, a greater proportion of the whole light was in a fit of reflection at the farther surface of the plate. But when any plate comes to be so thick, that the light of each denomination of colour has many parts reflected, and many parts transmitted, by reason that the whole contains many species, which shall have different numbers of fits of easy reflection or easy transmission; then the light reflected will not only be white, but the number of the species of light in fits of reflection will be so equal to the number of those in fits of transmission, that the proportion between the quantity of reflected and transmitted light will not be varied by any increase of the thickness of the plate.

15. For instance in the plate of air before mentioned the 800th part of an inch in depth the extreme red light in the middle of a fit of easy transmission at the first surface, would arrive at the farther surface in the middle of the 99th fit of easy reflection, and the extreme violet in the middle of a fit of easy transmission at the first surface, would arrive at the farther surface rather beyond the verge of the 156th fit of easy transmission; insomuch that, as there would be at least 57 different species of rays, which would arrive at the farther surface after a different number of fits of reflection, so there could not be reckoned above one less in fits of transmission. Were the plate the tenth part of an inch in thickness, the number of species arriving at the farther surface after different fits of reflection would be no less than 4629, and the number of species in fits of transmission cannot at most differ from this above one. Hence it comes to pass, that in all thick plates, and consequently in all the common appearances, the light reflected

reflected from every point of either surface of colourless transparent substances, whatever be the figure of their superficies, is not only always white, but also the same in quantity of light, the same proportion of the whole light arriving at each point of such surfaces in fits of easy reflection.

16. HENCE appears how absolutely this gentleman has erred in this whole matter, when, in order to account for the double, or more compounded appearance of objects seen indistinctly he imagined, that by reason of these alternate fits of easy reflection and transmission whenever a pencil of rays, from a distant object falls upon the cornea; that because some parcels of its rays will be transmitted into the eye, while other parcels are reflected back into the air, therefore, if the light were received upon a plane placed before the crystalline, it would consist of a middle circle surrounded with rings dark and luminous alternately.

17. NEITHER can any diversity be made, as this gentleman farther supposes, in the transmitted light at either surface of the crystalline; for the brightest part of the light in passing between the cornea and crystalline, which is the least of the two intervals, and that wherein the fits are longest, will have more than 12000 fits of reflection, or refraction.

18. BUT as little as this gentleman appears to have understood the subject, he had far other thoughts; for he has not contented himself with applying this curious doctrine, as he terms it, to vision, but even takes upon him to correct and improve upon Sir Isaac Newton by making the fits of reflection of different extent from the fits of transmission in a manner contrary to what Sir Isaac Newton has represented. From what we said above, it appears, that in common half the light incident upon any surface is in a fit of transmission, and the other half in a fit of reflection. Hence arises this obvious question, Why

is not half the light reflected at all surfaces? But in regard to this it must be considered, that Sir Isaac Newton has pursued his speculations upon this subject no farther, than what he could determine by indubitable experiments and observations, and he has found, that at all surfaces, where the light is acted upon by bodies, as a part is transmitted into pellucid substances, so a part is reflected; and he likewise finds, that as the transmitted light is diverted from its straight course by a refractive power, where that power is strongest, the greatest quantity of light is reflected. From this and other considerations he concludes, that it is one and the same power, by which the light is refracted and reflected; and that the light, which is reflected, is only such, as is in a fit of easy reflection, when it arrives at the surface; and as these fits in the passage of the ray to the transparent substance are alternately changed for fits of transmission, and return at certain intervals; so each fit likewise continues for a certain space, and that the intervals of these returns, and the space, through which each continues, depend altogether upon the medium, through which the light passes, and not upon that, whereon the light falls. But as Sir Isaac Newton had not discovered, wherein this power in bodies of refracting and reflecting light expressly consists; so he is silent in relation to the cause, why more light is reflected from some bodies than from others.

19. Now this gentleman imagines this defect in Sir Isaac Newton may be supplied by correcting him in regard to the extent of each fit of reflection, and supposing that to be varied according to the medium, upon which the light falls; for instance, when the light passing through air upon glass, or through glass upon the air, the extent of the fit of easy reflection should not be what Sir Isaac Newton supposes, half the interval between the middle of the nearest fits of

reflection, but only $\frac{11}{100}$ of that interval, and at surfaces between air and water $\frac{6}{100}$ of that interval.

But this supposition is absolutely inconsistent with the very experiments, from which Sir Isaac Newton deduces his whole doctrine. If the extent of these fits varied thus, not only the quantity of light reflected from a transparent surface would be varied by changing the contiguous medium, but also the colour, which, Sir Isaac Newton says, he could never observe. The colours, which arose successively upon the bubble of water, Sir Isaac Newton has described very distinctly with all the concomitant circumstances; but were it examined according to the new regulation of our author, what colours ought to have appeared, we shall find, that from the yellow, which Sir Isaac Newton ranges in the 5th order of colours, to the total blackness, each colour ought to emerge almost uncompounded, being, where the least simple, scarcely mixed with the contiguous colours of the same order, and never with those of any other. In particular the purple in the 5th order, which, Sir Isaac Newton says, was much inclined to red, must have been a pure violet without any mixture, and a dark interval have been seen between that and the red of the next order; even in the 7th order, where they are most compounded, no thickness of the bubble can be assigned, at which more than three colours will be reflected; insomuch that in no part of this 7th order of colours any whiteness could appear: nay, were the extent of the fit of easy reflection any thing less than what, Sir Isaac Newton supposes, a sensible interval void of reflection must have appeared between this and the order preceding. This shews, that the proportion assigned by Sir Isaac Newton between the extent of each fit of easy reflection and the interval of the fits was the result of
mature

mature deliberation, and necessary towards producing the appearances.

20. HENCE it appears, that whether more or less light be reflected from a body, the extent of the fits of reflection in the incident rays is the same, and the difference in the quantity of reflected light is owing to the greater or less portion of that part of the light, which arrives at the body in a fit of easy reflection, being thrown back by the reflective power of the body. The cause this gentleman has been pleased to assign for this, is so absolutely inconsistent with the essential principles of Sir Isaac Newton's doctrine, that the thought could never have been entertained by one of the least degree of skill in the subject. But this gentleman seems so great a stranger to the matter, he has writ upon, that he does not appear in the least to have known, that rays of different colours have their fits of reflection and transmission of different lengths. For from this it follows, that, when a thin transparent plate, whether the bubble of water in Sir Isaac Newton's experiments, or the plate of air between the flat and convex glass, is exposed to the open light of the day, scarce any part of it is exempted from reflecting light, except the central spot only, but the rest appears spread over with different colours contiguous to one another. But had this gentleman been at all apprized of this, he should at least have suspected, that if the phænomenon, he is here endeavouring to account for, depended upon the cause, he assigns for it, instead of a multitude of separate images a broad one should have presented itself variegated with colours.

21. AFTER it has been thus made appear, how totally unskilled this gentleman is, in this doctrine relating to the fits or dispositions of easy reflection and of easy transmission accompanying the rays of light; I know not, whether it may be thought worth

while to take notice, how little this gentleman's erroneous opinions concerning it will serve his purpose. By computing on supposition that rays of all colours have the same intervals between their fits, and the extent of each fit what this gentleman assigns, the magnitude and number of the rings, that are formed at the cornea, when the rays are incident in such a manner as to make the rings the largest, will be above 670, if the aperture of the pupil be supposed $\frac{1}{10}$ of an inch; and if the magnitude of the first or largest dark ring be desired, it will be found, that, if the rays falling on the middle of the cornea be in a fit of easy transmission, the breadth of the first dark or reflected ring will be about the $\frac{1}{500}$ of the whole pupil; but if the rays falling on the middle of the cornea, be in a fit of easy reflection, then the breadth of the first dark ring will be but about the $\frac{1}{660}$ part of the whole pupil; if we conceive now the vision to be so indistinct, that a luminous point appears under an angle of 20 minutes (a degree of indistinctness five times greater than that, under which he supposes these rings to be perceptible) the breadth of the first or broadest dark ring cannot, under all these favourable circumstances, subtend a greater angle at the eye than $2\frac{2}{3}$ seconds; and consequently none of these rings could, even on his own representation of this doctrine, be ever sensible. It is evident then, that, when our author, to account for the double appearance of objects seen by indistinct vision, supposes the visibility of these rings, it was not only necessary, that he should utterly misapprehend Sir Isaac Newton's doctrine; but that he should also be ignorant of the most obvious consequences of those absurd conceptions, he had formed to himself on this subject.

22. IN the last part of this essay our author has discoursed very diffusely upon the dazzling, to which the eye is subject from the immediate contrast of light and darkness, and tells us, he should not have been so particular, had he not suspected, that Sir Isaac Newton was once led into a mistake by an appearance similar to those, he here treats of; at least, says this gentleman, I do not find, he has any where accounted for the extraordinary strength of the ring of light next the central black spot spoken of in Observation 23, Part 1. of his second book of Opticks; nor do I see any way of accounting for it, but from some such consideration.

23. HERE it is supposed, first, that Sir Isaac Newton, in attributing any superior degree of brightness to this ring, was deceived by the neighbourhood of the central black spot; secondly, that Sir Isaac Newton has no where accounted for this extraordinary brightness; and lastly, that no account can be given of it, but what our author here assigns.

24. IF this were indeed a mistake of Sir Isaac Newton, it must be owned a very signal one; since he so little suspected this appearance to be owing to any such incidental cause, that when he is ranging the colours of bodies under the several rings produced in these experiments, he places the light of white metals under the class of this ring, on account of their superior brightness. But it is further evident, that he was not imposed upon by any such deception, from the method he took to satisfy himself of the extraordinary brightness of the light of this ring, by comparing two bubbles of water blown at distant times; in the first of which the whiteness appeared, which succeeded the colours, and in the other the whiteness, which preceded them all*; and before † he tells us, this whiteness, which succeeded the colours, would often spread and dilate itself over

* Opticks, pag. 196.

† Ibid. pag. 190.

the greater part of the bubble, before that even the succeeding blue emerged on the top; nay, would decay gradually, and quite vanish, before the black spot appeared.

25. AGAIN, though Sir Isaac Newton has not separately discussed the cause of this particular appearance, yet it is so unnecessary to have recourse to this gentleman's chimerical imagination concerning it, that it is a most obvious consequence from the method, Sir Isaac Newton gives for investigating the several colours exhibited by thin plates. For it thence appears, that at this thickness of the plate no part of the light, which enters it, arrives at its farther surface in a fit of easy transmission, which never happens in any greater thickness.

26. THUS then has this learned gentleman unhappily attempted the application and improvement of a doctrine, of which, it appears, he was not qualified to describe even the very rudiments.

27. FOR the first principle laid down by Sir Isaac Newton, in the second part of his second book of Opticks, is the inequality of the length of the fits of transmission and reflection in rays of different colours, and it is only from the combinations of these different lengths in plates of a different thickness, that all the appearances described in the first part are deduced.

28. BUT the description and application of this doctrine, as given us by our author*, do necessarily suppose the lengths of these fits to be the same in rays however different in colour.

29. AGAIN, Sir Isaac Newton in the same ray makes the fit of reflection equal in length to the fit of transmission, the experiments in the first part necessarily requiring it.

30. ON the contrary Dr. Jurin tells us, that in water the fit of reflection is in length but $\frac{6}{94}$ of the

* Art. 209, 210, 211, &c.

fit

fit of transmission; an assertion that absolutely destroys the whole theory established by Sir Isaac Newton, setting it at variance with almost every appearance, it is intended to solve.

31. LASTLY, The immediate consequence of Sir Isaac Newton's theory, as described in the second book of Opticks, is, that the light of the first ring surrounding the central spot should be much stronger than that of the succeeding rings, all the rays being there in a fit of reflection.

32. YET so little was our author versed in this doctrine, that he conceives the remarkable strength in the light of this ring to be only an illusion, suspects Sir Isaac Newton to have been mistaken in his description of it, and tells us, that he does not see, that its reality can be any way accounted for.

* * * THUS Mr. Robins shewed the fallacy of what had been advanced in the Essay upon distinct and indistinct vision. But its author was not a man to be convinced even by demonstration; for he published a Reply, whereof Mr. Robins soon printed a Full Confutation; there exposing also the many errors he had further committed. But the above Remarks are abundantly sufficient to prevent any intelligent person from being imposed on by Dr. Jurin's Essay, or by whatever may be urged in its defence. For the Doctor laboured in vain to account for an appearance, that depended solely on mixt and heterogeneous light, from a property of the most pure and unmixt light, such as exists no where. I shall therefore only give two paragraphs from Mr. Robins's last Treatise, as they contain a certain proof of a particular, he had delivered merely as a conjecture.

“ To spare this gentleman (Dr. Jurin) the
 “ labour of framing a new hypothesis, and be-
 “ wildering himself further in this subject, I will
 “ inform him of some particulars relating to the
 “ multiple appearance of objects seen indistinctly,
 “ that will not only evince, that the fits of trans-

“ mission and reflection have nothing to do in this
 “ affair, but will also prove, that the cause, I had
 “ hinted at in my Remarks *, is undoubtedly the
 “ genuine one.

“ It is true, that a narrow line of light, such
 “ as the interval between the edges of a parallel
 “ ruler brought very near together, will appear
 “ bordered with other lines of light, when held
 “ either too near or too remote for distinct vision.
 “ If Dr. Jurin’s assignment of the cause of this
 “ appearance can at all take place, the stars ought
 “ not to appear radiated, but invironed with con-
 “ centric circles of light; as the forementioned line
 “ of light is bordered with parallel lines. A very
 “ small pin-hole in a piece of paper being held up
 “ against the light, first at a distance from the eye,
 “ wherein it may be seen distinctly; and then, while
 “ the eye is directed to it, drawn gradually nearer;
 “ this hole, as soon as it ceases to be seen distinctly,
 “ will also appear radiated like a star, not incompassed
 “ with luminous circles. If the appearances in this
 “ experiment are duly attended to, they will account
 “ for the lines of light in the other observation: for
 “ these rays will most of them, if not all, have a lucid
 “ speck in them; so that if a slit were cut in the pa-
 “ per, through the pin-hole, of the breadth of the
 “ hole, each of these specks will become a line of
 “ light parallel to the slit. But circles of light not
 “ appearing about the pin-hole in this experiment,
 “ shews plainly, that no such luminous circles are
 “ formed in the bottom of the eye, by rays issuing
 “ from a luminous point, either beyond, or within,
 “ the limits of distinct vision †.”

* Remarks on Dr. Jurin, §. 3. p. 280. l. 16.

† Robins’s Full Confutation of Dr. Jurin’s Reply, pag. penult.

APPENDIX

BY THE

PUBLISHER.

AS my friend, besides the abusive treatment, which he received from Philalethes Cantabrigiensis, has not escaped censures from other quarters; I here purpose to vindicate his memory from those aspersions. This will lead me to obviate some late insinuations against Sir Isaac Newton himself. And I shall farther make a few observations on that great man's early writings; having been enabled from papers, that have fallen into my hands, to determine more fully, than has been done, not only the order, in which the several tracts, he had from time to time composed, were writ; but also, a point which I conceive to be of much greater consequence, by what steps, he gradually corrected the crude ideas, he had received of indivisibles; till he perfected the doctrine, which Mr. Robins has distinguished himself by explaining.

Dr. Robert Simson, the learned professor of the mathematicks in the university of Glasgow (whom I must

must always honour for his zeal towards reviving a just taste in geometrical subjects) at the end of the second edition of his *Conick-Sections*, printed at Edinburg in 1750, after having given the solution of a celebrated problem of Alhazen, has surprized me by the following insinuation against Mr. Robins's candour, "Solutionem hanc, tam analysin quam compositionem, diu ante annum 1729 inveni; ex eo autem anno discipulis eam quotannis prælegi, iisque etiam scripto communicavi. Anno autem 1739 vir clarissimus Benjaminus Robins, in libello cui titulus, *Remarks upon Mr. Euler's Treatise of Motion*, p. 75, 76, &c. * compositionem a præcedente non multum abludentem publicavit. Num eam ipse invenerat. aut aliunde acceperat, non dicit vir doctissimus. Verisimile autem est eum analysin hujusmodi problematis, ope cujus ad compositionem retrogradaretur, non habuisse; dicit enim problema in multis casibus magis complicatum esse, quam ut solutionem simplicem et elegantem accipiat. Præcedens vero analysis, sive solutio æque simplex est, ac compositio ex ea derivata, eamque excogitaveram diu antequam Slusii constructionem videram; et in compositione analysin ita presse secutus sum, ut non observaveram rectam LM parallelam esse junctæ BC, ante visam Slusii constructionem. Hæc autem monenda duxi, ne quis putet me tantum analysin præcedentem Slusii constructioni aptasse."

HERE Dr. Simson has in the first place wholly mistaken Mr. Robins in regard to the problem, which he speaks of as too complex in most cases to admit of a simple and elegant solution; for Mr. Robins does not say this of Alhazen's problem, which he had not yet mentioned, but has reference to the proposition of Dr. Smith there animadverted

* Remarks on Dr. Smith, §. 64.

on, in its full extent; and Dr. Smith's proposition is this: "When a large picture, or a plane object of any given shape, stands perpendicularly to the common axis of any number of refracting or reflecting surfaces, its apparent shape, situation, magnitude and distance from the eye, at any point of the axis, may be found as follows *."

And Mr. Robins charges the explanation of this proposition with being defective, because there is no notice taken, that "the place of the image cannot be determined, as here required, till it be shewn, how to find the line, in which it is seen; that is, how to assign the reflected or refracted ray, which shall enter the eye †."

Though, to avoid the censure of unreasonable captiousness, he acknowledges, that "the problem, thus generally proposed, is, in most cases, too complex to admit of a simple and elegant solution;" but, however, thinks this no just excuse for Dr. Smith's passing over in silence, what solutions had been given of particular cases; and specifies two, this problem of Alhazen, and another concerning refraction given by Dr. Barrow in his Optical Lectures.

AGAIN, with respect to Alhazen's problem in particular, I am morally sure, Mr. Robins was wholly unapprized of Dr. Simson's having ever had it under consideration. For whenever Mr. Robins was in London, there was scarce a day passed, wherein I was not in his company; and he could not have failed mentioning to me a thing of this kind. The demonstration published in his Remarks, had been communicated to him by me from the papers of my friend Dr. Pemberton ‡, who had con-

* Compleat System of Opticks, book 2. chap. xx. prop. 1.

† Remarks on Dr. Smith, §. 64.

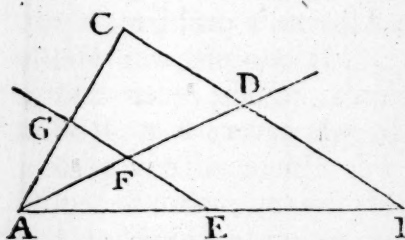
‡ This the Doctor permitted Mr. Robins to publish, but not to make use of his name.

sidered

sidered this problem above 40 years ago, when we were fellow students at Paris, upon the following occasion.

THE treatise of Apollonius De Sectione rationis having then lately fallen into his hands, he was greatly struck with the elegance of the ancient method of seeking the solution of problems, delivered there more fully and explicitly, than in any of their works, which had before been known to the modern world; though several specimens of their methods, scattered about in those writings, had enabled some to discover in part this ancient form of analysis. My friend's admiration of this method, induced him frequently to apply it to the problems, he met with solved by the calculations of algebra. And in particular, turning over Sir Isaac Newton's treatise on Algebra, with the intention of considering his problems in this light, where, among others, this occurred. To find the locus of the vertex of a triangle, wherein the angles at the base should have a given difference; he applied to this the ancient method of analysis in the following manner.

SUPPOSE, in the triangle ACB, the angle CAB exceeds the other angle at the base CBA, by the



given angle BAD. Then the angle CAD will be equal to CBA, and the triangles CAD, CBA similar; whence BC is to CA as AC to

BCD, and the rectangle under BCD equal to the square of AC. Hence if AB be bisected by EFG parallel to BC, the rectangle under EGF will be equal to the square of AG, or of GC. Consequently the point C will be in an equilateral hyperbola, in which EFG will be a diameter, E the center, CA ordinately applied to that diameter, and AF a tangent to the hyperbola in

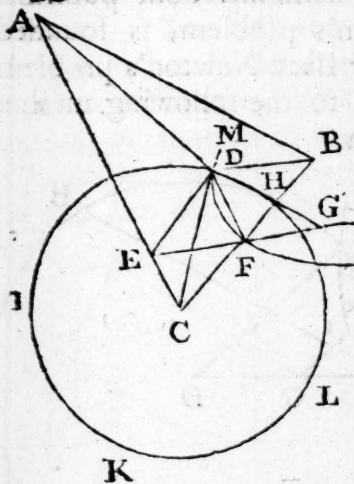
trifected, shall be a tangent in the point A; then the arch AF, included within this hyperbola, is one third of the arch AB.

DRAW the chord of the arch AF, divide AD into two equal parts at G, so that G be the center of the hyperbola, join DF, and draw GH parallel to DF, cutting the chords AB, AF in I and K; then the hyperbola being equilateral, every diameter is equal to its latus rectum, and the rectangle under GKI will be equal to the square of AK, that is, GK will be to KA as AK to KI, the triangles GKA, AKI similar, and the angle KAI equal to AGK, which equals the angle ADF. Now the angle ADF, at the center of the circle, being equal to KAI at the circumference; the arch AF will be equal to half FB, and therefore equal to one third of AB.

At the same time, his determination of this locus suggested to him, how easily it might be applied to Alhazen's problem: since, if from the point D (I refer to Mr. Robins's figure) right lines were drawn to the points E and F, assigned according to Huygens and Slufius, the angles CED, CFD would, as the Marquis de l'Hôpital had observed*, be equal, whereby EF being joined; the angles DEF, DFE would have the same difference, as the angles CEF and CFE in the given triangle ECF.

AND now I hope, from what has been said, Dr. Simson will no longer retain any suspicion, that Mr. Robins could be guilty of an act of meanness, whereof (if I may be allowed to know

* Sections Coniques, Liv. x. Exempl. vii.



the temper and disposition of one, with whom I conversed so intimately for many years) I may pronounce him utterly incapable in any occurrence of life whatever.

BUT I must beg leave further to add, that though Dr. Simson's analysis leads very directly to the demonstration published by Mr. Robins; yet, if exhibited as an adequate solution of the problem, it is incomplete. We see by the treatise above mentioned of Apollonius, that the custom of the ancients, in their analysis, was to inquire into all the various cases of a problem, and the limitations, to which the solutions in each were subject; and Dr. Simson has justly observed, that " sine determinationibus, five διορισμοῖς, problema rite solutionum minime habendum est *." But here Dr. Simson has imitated Huygens and Slusius in attending only to the general solution of this problem, as Dr. Barrow had done before in his Optical Lectures; whereas Alhazen has taken into consideration the several cases, though in some measure unsuccessfully.

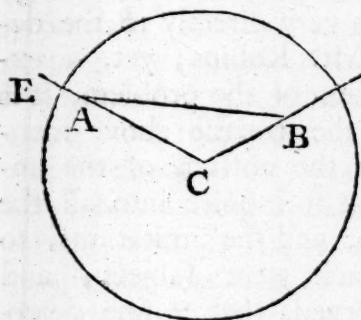
THE problem admits of solution only within the angle ACB, and in the angle vertical to it, comprehended by the lines AC, BC continued backwards *. Therefore, when both the points are given without the speculum (though the hyperbolas cut the circle in four points) they exhibit but two solutions only; one on the convexity, where the hyperbola, whose vertex in Mr. Robins's figure is F, passes through the surface of the speculum within the angle ACB; and another in the concave part, where the opposite section to this hyperbola, that whose vertex is E, passes through the speculum within the angle vertical to that under ACB.

BUT when one of the points, and more especially, when both are given within the speculum,

* Preface to his Conics, p. vii. † Alhazen, lib. 5. prop. 66.

the problem is subject to a greater variety of solutions.

IN relation to the hyperbola, whose vertex is E, as it passes through the center of the speculum, it

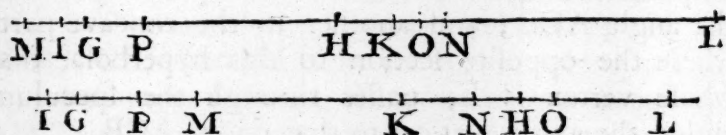


must always exhibit one solution within the angle vertical to ACB; but when the point A shall be within the speculum, as in the annexed scheme, the point E shall be without, and this hyperbola shall

enter the speculum within the angle ACB, and exhibit another solution of the problem within that angle.

IN the next place, in regard to the hyperbola, whose vertex is F, when B is within the speculum, F is without. And here it is evident, that if the angle ABC is either obtuse, or right; the hyperbola, whose vertex is F, cannot meet the speculum within the angle ACB, whether the point A be within or without the speculum; since the tangent of this hyperbola in the vertex F, being parallel to AB, must pass on this side wholly without the speculum.

BUT when the angle ABC is acute, if GH be a third proportional to BC and the semidiameter of the speculum, and GI being taken to IH in the duplicate ratio of the difference between AC and



CB to their sum, IK be taken equal to AC, and prolonged to L, that the ratio of IK, that is AC, to KL be the duplicate of four times the rectangle under

under AC, CB to thrice the rectangle under their sum and difference; and if GM, equal to HK, be added to HG, when IK exceeds IH, otherwise taken from it, and to IK be added KN, a third proportional to thrice ML and KL, and NO, which with ML shall make a rectangle equal to that under GKH, being taken from IN, when IK exceeds IH, otherwise added to it; then if OP, taken from O towards G, shall be to LP, as the square of LP to thrice the rectangle under KLM, GP will be the versed sine, to the diameter GH, of half the angle ACB, when the hyperbola, whose vertex is F, touches the speculum.

BUT from this analogy LP, and consequently GP, may be easily found by the methods too well known to make any particular explanation here necessary.

ONLY it must be observed, that when two points are found by this analogy within GH, that nearest to G gives the versed sine, which determines the angle ACB.

IF AC is less than IG, or if the angle ACB be less than this now found, the hyperbola, whose vertex is F, will not reach the speculum within the angle ACB; but if greater, this hyperbola will pass through the speculum, and exhibit two solutions of the problem.

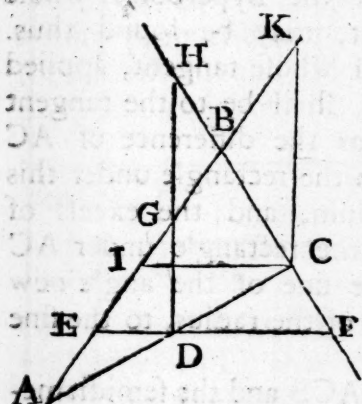
ON the other hand, if the angle ACB is given, with the two distances AC, CB; the semidiameter of the speculum, wherein the hyperbola, whose vertex is F, will touch it, may be found thus. Find an angle, the cube of whose tangent, applied to the square of the radius, shall be to the tangent of half the angle given, as the difference of AC from CB to their sum; then the rectangle under this semidiameter of the speculum, and the excess of AC above CB, will be to the rectangle under AC and CB, as the cube of the sine of the angle now found, applied to the square of the radius, to the sine of half the angle given.

MOREOVER, if the angle ACB and the semidiameter

ter of the speculum were given, together with one of the distances AC or CB; the other may be assigned, wherein the hyperbola, whose vertex is F, shall exhibit a single solution by touching the speculum.

To set down at length the investigation of all these particulars, would exhibit an extensive example of applying the ancient analysis to the determining the limits of the higher order of problems; of which Dr. Pemberton has long ago given a very distinct specimen, in the two problems concerning the rainbow above referred to. But as this would carry me too far from my principal design; let it suffice here to observe upon the whole, that as the point A may be either without the speculum, within, or on the surface, while the point B is within; when B and A are both within the speculum, the problem may admit of two, three, or four solutions; for two will be given by the hyperbola, whose vertex is E; but when the point A is in the surface of the speculum, or without, this hyperbola will exhibit but one solution, and the problem may have that single solution only, or by the concurrence of the other hyperbola have, in the whole, three solutions, or perhaps only two.

AND thus I shall take leave of this problem, after adding the two following short remarks.



In the first place, Huygens's construction of this problem will admit of a concise demonstration by the following proposition.

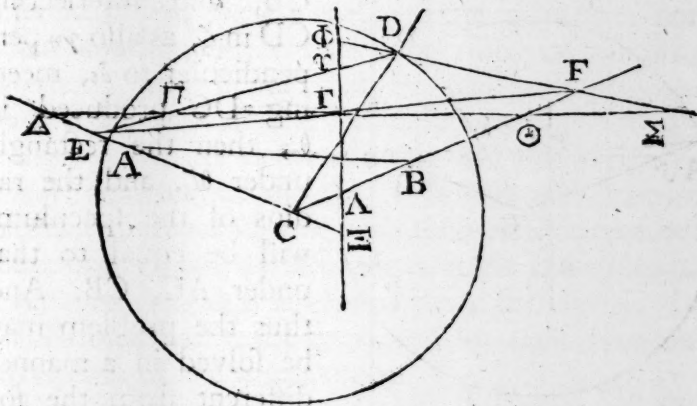
In any triangle ABC, if the base AC is bisected in D, and EDF, DGH be drawn, each making equal angles with the legs AB, CB, the

the four segments AE, CF, BG, BH will be all equal.

For CI being drawn parallel to EF, BI will be equal to BC and the difference between AB and BC, and AE, EI each equal to half that difference, being equal to each other, since AD is equal to DC: but CF is equal to EI; therefore AE and CF are equal.

In like manner if CK is drawn parallel to DH, BK is equal to BC, and BG half the difference of AB, BK, being also equal to BH, as the angles BGH and BHG are equal; therefore GB and BH are each equal to AE and CF.

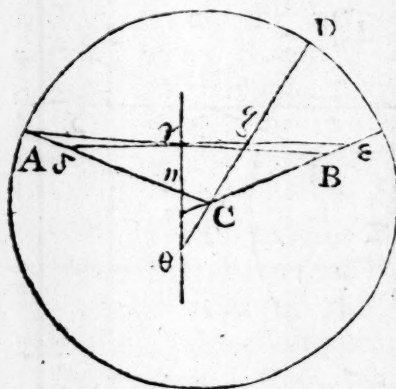
Now Huygens's construction is this. The line EF being bisected in Γ, and the lines ΔΓΘ, ΓΛΞ



drawn to make each of them equal angles with EC, CF, the opposite hyperbolas having these lines for asymptotes, and one of them passing through the center C, will be intersected by the surface of the speculum in the points, wherein the reflection sought should be made. Here if D be the point, where the reflection is made, the angles CED, and CFD are equal; and EF being bisected in Γ, and ΔΓΘ, ΓΛΞ making each equal angles with CE and CF, if ΔΘ intersect ED in Π, and DF produced in Σ, also ΓΛ intersecting ED in Γ, and FD in Φ, in the triangles ΔΕΠ, ΘFΣ, the angles ΔΕΠ, ΘFΣ are equal, also

the angles $\angle E\Delta\Pi$, $\angle \Theta\Sigma F$ are equal; consequently the angles $\angle \Delta\Pi E$, $\angle \Theta\Sigma F$ are equal, and $\Pi\Sigma$ drawn through Γ , wherein EF , being the base of the triangle EDF , is bisected, makes equal angles with the legs ED , DF . In like manner, the angles $\angle E\Xi\Upsilon$, $\angle \Lambda\Phi$ being equal, the angles $\angle \Upsilon\Gamma F$, $\angle \Phi F \Gamma$ are equal; whence, by the preceding proposition, $E\Pi$, ΥD , ΦD , $F\Sigma$ are all equal: therefore the points E , D , and F are in opposite sections passing through E and F , having for asymptotes $\Delta\Theta$, $\Gamma\Lambda$: and in the triangle ECF , ED being equal to $C\Sigma$, the hyperbola through E passes also through C .

I SHALL also further add, that if AB be bisected in γ , and $\delta\epsilon$ be drawn making equal angles with AC , CB , and intersecting



CD in ζ , as also $\gamma\theta$ perpendicular to $\delta\epsilon$, meeting DC produced in θ ; then the rectangle under $\theta\zeta$, and the radius of the speculum, will be equal to that under AC , CB . And thus the problem may be solved in a manner different from the so-

lutions both of Huygens and Slusius, or the original of Alhazen, by applying a line, as $\theta\zeta$, which shall be to one of the lines AC , CB , as the other to the radius of the speculum, through the point C , within the lines $\delta\epsilon$, $\gamma\theta$, given in position.

I SHALL now proceed to defend my friend from reflections, that come from another quarter.

THOUGH Mr. Robins, in his Tracts on fluxions and prime and ultimate ratios, had given a very just and clear account of their nature and use, conformable to the sense of their great inventor; yet, it seems, it was so different from the notions, many had entertained

tained of these speculations, that divers objections were made to what he had published. Amongst other things it was pretended, that the way of demonstrating followed by the ancient geometers, so much commended by him, was tedious and perplexed, if not inconclusive; that he had done ill in distinguishing the method of fluxions, from that of prime and ultimate ratios *; that he had been presumptuous in saying Sir Isaac Newton was too brief in his explications †; that he had explained wrong Sir Isaac Newton's expressions of the prime and ultimate ratios of the quantitates nascentes and evanescentes, by having maintained, that those prime and ultimate ratios were only the limits of all the different ratios, these variable finite quantities bore to one another, while they were rising or vanishing together; and hence that he had misrepresented the first Lemma of the Principia, by endeavouring to shew, that its demonstration did not prove the actual coincidence of the inscribed and circumscribed rectilinear figures with their respective curvilinear limits, and by asserting that Sir Isaac Newton never intended such a coincidence; and lastly, that Mr. Robins had quite perverted Sir Isaac Newton's description of the momentum.

THE gentleman, who under the name of Philalethes Cantabrigiensis had undertaken a defence of Sir Isaac Newton against Dr. Berkeley the author of the Analyst, not brooking, that Mr. Robins's explanation of these doctrines should be inconsistent with his own, refrained not from casting out against him these and other reproaches; all which might indeed, nay perhaps ought to have been, passed over without any notice, as solely the effects of anger and disappointment, and as having been already answered by Mr. Robins himself; had not

* Account, §. 40. Dissertation, §. 9, 10. † Discourse, §. 4, 94. Account, §. 28.

the behaviour of Philaethes, in these contests, been seriously commended, and Mr. Robins as much censured, by M. de Buffon, in the Preface to his Translation of Sir Isaac Newton's Treatise of Series and Fluxions, printed at Paris in 1740, with a train of general invective, without citing any one expression from Mr. Robins's writings in support of it; merely, as it seems, from the high notions he had been inspired with of Philaethes, and his manner of writing; for mentioning his Tracts against the author of the Analyst, M. de Buffon is pleased to say, "qui sont admirables pour la force de
"raison et la finesse de raillerie qu'on y trouve par
"tout—." But perhaps, when he calls to mind a small pamphlet, entitled *Lettre à Monsieur de Buffon par M. Jurin*, writ in his own language, he may not be unwilling, that this high strained compliment should be ascribed to his want of a perfect knowledge in ours.

I AM surprized at M. de Buffon's imagining, that the author of the Analyst could find any cause of triumph in the representation Mr. Robins had given of Sir Isaac Newton's doctrine, which at once destroyed all his objections; while Philaethes by allowing, at least in part, Dr. Berkeley's interpretations of Sir Isaac Newton, denied the consequences in vain; and was so far from demolishing the Doctor's writings, or humbling his metaphysical pride, as M. de Buffon expresses it *, that Philaethes left him room enough to write on without end; but on the appearance of Mr. Robins's book, the author of the Analyst thought fit to be silent.

BUT M. de Buffon gave so little attention to what he scrupled not to treat with so much ill manners, that he asserts of Mr. Robins, that "il avouë que
"la géometrie de l'infini est une science certaine,
"fondée sur des principes d'une vérité sûre—;"

* Preface, p. xxvii,

whereas Mr. Robins always contended, that what is styled the geometry of infinites was no certain science, nor built on sure principles; and therefore he has shewn, according to Sir Isaac Newton, that the proportions of fluxions are not the proportions of any quantities imagined to be infinitely small, but only the limits of the varying ratios of finite quantities, that diminish together, till they vanish.

WITH the same precipitancy M. de Buffon concludes by asserting, that the mathematicians paid no regard to what Mr. Robins had said ("il n'y a pas eu moyen de leur faire croire un seul mot de tout cela.") But the contrary is evident; for the best writers soon after trod in Mr. Robins's steps, and expressed themselves conformably to his sentiments and phraseology without reserve.

EVEN M. de Buffon's own countrymen are at length come into Mr. Robins's way of explaining these subjects; for M. de Bougainville, in the Preface to his *Traité du calcul integral* *, says, "Le calcul de Newton est independant de la réalité des quantités infiniment petites—." And M. d'Alembert has, at several times, shewn his dislike to the notion of infinitely little quantities. In his *Traité de Dynamique* †, speaking of the *Méthode des infiniment petits*, he says, "Que les commentateurs qui n'en pénètrent pas toujours l'esprit, pourroient s'accoutumer à regarder ces infiniment petits comme des réalités; c'est une erreur contre laquelle on doit être d'autant plus en garde, que de grands hommes y sont tombés, et qu'elle-même a donné occasion à quelques mauvais livres contre la certitude de la géométrie." In Tom. I. of the *Encyclopédie* ‡ he has these words, "Il n'y a point réellement de quantités infiniment petites,—. Cette idée des *limites* est très-nette et très-

* Printed at Paris in 1754. p. viii.

† Ibid. in 1743.

p. 36.

‡ Ibid. in 1751. p. 845.

“ utile pour reduire la geométrie des infiniment
 “ petits à des notions claires.” And in Tom. IV. *
 “ Il [Newton] n’a jamais regardé le calcul *différen-*
 “ *tiel* comme le calcul des quantités infiniment pe-
 “ tites, mais comme la méthode des premières et
 “ dernières raisons, c’est-à-dire la méthode de trouver
 “ les limites des rapports.”

BUT more particularly Mr. Maclaurin published, within seven years after Mr. Robins, his Treatise of Fluxions at Edinburg in 1742; wherein he conformed himself entirely to Mr. Robins’s sentiments, in regard to Sir Isaac Newton’s doctrine. And though he was joined in a long course of intimacy with the chief of Mr. Robins’s opposers, and personally unknown to him; yet in the preface he has publickly commended Mr. Robins’s performance.

NAY, Mr. Maclaurin not only concurs with Mr. Robins in his interpretation of these doctrines, but has even expressly followed his plan in treating the subject.

Mr. Maclaurin in the Account he gives himself of his book in the Philosophical Transactions, N^o 468, p. 325, thus begins. “ The author’s first
 “ design, in composing this Treatise, was to establish
 “ the method of Fluxions on Principles equally
 “ evident and unexceptionable with those of the
 “ ancient Geometicians by Demonstrations deduced
 “ after their manner in the most rigid form †, and
 “ by illustrating the more abstruse Part of the Doc-
 “ trine, to vindicate it from the imputation of un-
 “ certainty or obscurity.” Again, in the introduc-
 “ tion to his book, p. 3. Mr. Maclaurin says, “ The
 “ method of demonstration, which was invented by
 “ the author of fluxions, is accurate and elegant ‡;
 “ but we propose to begin with one that is some-
 “ what different; which, being less removed from

* Printed at Paris in 1754. p. 985, 986.

§. 40, 151. Account, §. 6.

† Discourse,

‡ Discourse, §. 3, 94, 142, 150.

“ that

“ that of the ancients, may make the transition to his
 “ method more easy to beginners, and may obviate
 “ some objections that have been made to it *.”

ALL this is an exact description of the method Mr. Robins pursued. For though Mr. Maclaurin has expressed himself thus tenderly, yet his method does not indeed differ essentially from that of the ancients.

He immediately subjoins, “ But, before we proceed, it may be of use to consider the steps by which the ancients were able, in several instances, from the mensuration of right-lined figures, to judge of such as were bounded by curve lines †: for as they did not allow themselves to resolve curvilinear figures into rectilinear elements, it is worth while to examine by what art they could make a transition from the one to the other ‡.” This also is the very order in which Mr. Robins proceeded in regard to the doctrine of prime and ultimate ratios. Mr. Maclaurin farther adds, that “ as they were at great pains to finish their demonstrations in the most perfect manner, so by following their example, as much as possible in demonstrating a method so much more general than theirs, we may best guard against exceptions and cavils, and vary less from the old foundations of geometry.” And he illustrates this by the very example, which Mr. Robins § had given before him, in explaining the methods of the ancients. Thus [p. 8.] “ when Archimedes demonstrated, that the area of a circle is equal to a triangle upon a base equal to the circumference of the circle, of a height equal to the radius, it was not by supposing it to coincide with a circumscribed equilateral polygon of an infinite number

* Discourse, §. 40, 151. Account, §. 6.
 §. 89. Dissertation, §. 12, 103.
 ¶ Discourse, §. 90.

† Discourse,
 ‡ Ibid. §. 13.

“ of sides, but in a more accurate and unexceptionable manner.”

MR. Maclaurin so perfectly approved of the course Mr. Robins took of justifying Sir Isaac Newton's doctrine, by comparing it with the practice of the ancients, that he chose to expatiate largely in describing their methods, and in comparing them with the practice of the moderns before Sir Isaac Newton.

THUS at p. 33. “ It is often said, that curve lines have been considered by them as polygons of an infinite number of sides. But this principle nowhere appears in their writings. We never find them resolving any figure or solid, into infinitely small elements. On the contrary, they seem to avoid such suppositions, as if they judged them unfit to be received into geometry, when it was obvious that their demonstrations might have been sometimes abridged by admitting them. They considered curvilinear areas as the limits of circumscripted or inscribed figures of a more simple kind, which approach to these limits (by a bisection of lines, or angles, that is continued at pleasure *,) so that the difference betwixt them may become less than any given quantity †. The inscribed or circumscribed figures were always conceived to be of a magnitude and number that is assignable; and from what had been shewn of these figures, they demonstrated the mensuration or the proportion, of the curvilinear limits themselves, by arguments *ab absurdo* ‖. They had made frequent use of demonstrations of this kind from the beginning of the elements; and these are in a particular manner adapted for making a transition from right-lined figures to such as are

* Dissertation, §. 13, 107.

† The use made in geometry of the above expression *any given quantity*, Philalethes never could understand, which occasioned many of his mistakes. Review, §. 18. Dissertation, §. 42, 108, &c.

‖ Account, §. 28.

“ bounded

“ bounded by curve lines *. By admitting them
 “ only, they established the more difficult and sub-
 “ lime part of their geometry on the same founda-
 “ tion as the first elements of the science. Nor
 “ could they have purposed to themselves a more
 “ perfect model †.”

THIS is expressly contrary to what Philalethes con-
 tended for, in opposition to Mr. Robins, that Sir
 Isaac Newton's method, by proving the varying
 quantities came up to their limits, was more perfect
 than that of the ancients. Whereas Sir Isaac New-
 ton never claimed such superiority; and had Phila-
 lethes comprehended the ancient method of demon-
 stration, he would have known it to be impossible to
 excel it in point of evidence; and not have laid his
 friend Mr. Maclaurin under the necessity of thus con-
 tradicting him. Sir Isaac Newton contented himself
 with asserting, that his methods were consonant to
 that of the ancient geometers. The coincidence
 contended for, and thus highly praised by Phila-
 lethes, is the very essence of indivisibles.

BUT Mr. Maclaurin proceeds farther, and confirms
 all, Mr. Robins had said against the tediousness and
 perplexity, which Philalethes objected against the
 ancient mode of demonstration.

IN p. 35 he says, “ His [Archimedes] method
 “ has been often represented as very perplexed, and
 “ sometimes as hardly intelligible ‡. But this is not
 “ a just character of his writings, and the ancients
 “ had a different opinion of them. He finds it
 “ necessary indeed to premise several propositions to
 “ the demonstration of the principal theorems; and
 “ on this account his method has been excepted
 “ against as tedious. But the number of steps is
 “ not the greatest fault a demonstration may have;
 “ nor is this number to be always computed from
 “ those that may be proposed in it, but from those

* Dissertation, §. 12. † Discourse, §. 40. ‡ Dissertation, §. 53.

“ that

“ that are necessary to make it full and conclusive.
 “ Besides, these preliminary propositions are generally
 “ valuable on their own account, and render our
 “ view of the whole subject more clear and com-
 “ plet. In his treatise of the sphere and cylinder,
 “ for example, by his demonstrating so fully the
 “ mensuration of the surfaces and solids, generated
 “ by the internal and external polygons, we not only
 “ see how the surface and solid content of the sphere
 “ itself is determined, but we acquire a more perfect
 “ knowledge of this theory, and of all that relates
 “ to it, with a satisfaction * that we are sensible is
 “ often wanting in the incompleat demonstrations of
 “ some other methods.”

INDEED the perplexity of a demonstration does not
 arise from the number of steps in it ; but from the
 disorderly disposing those steps, and which are gene-
 rally increased from want of order. This Philalethes
 could never understand ; not being able to distinguish
 between a long and a perplexed demonstration.

THE consequences of the moderns, departing from
 this rigour in demonstration used by the ancients, in
 introducing infinites, are thus expressed by Mr. Mac-
 laurin at p. 38, 39. “ But when the principles
 “ and strict method of the ancients, which had hi-
 “ therto preserved the evidence of this science entire,
 “ were so far abandoned, it was difficult for the
 “ Geometricians to determine where they should stop.
 “ After they had indulged themselves in admitting
 “ quantities of various sorts, that were not assigna-
 “ ble, in supposing such things to be done as could
 “ not possibly be effected, (against the constant prac-
 “ tice of the ancients,) and had involved themselves
 “ in the mazes of infinity, it was not easy for them
 “ to avoid perplexity, and sometimes error †, or to
 “ fix bounds to these liberties when they were once

* Dissertation, §. 54.
 † Account, §. 30, 31, 32, 33.

† Account, §. 3, 38.

“ intro-

“ introduced. Curves were not only considered as polygons of an infinite number of infinitely little sides, and their differences deduced from the different angles that were supposed to be formed by those sides; but infinites and infinitesimals were admitted of infinite orders, every operation in geometry and arithmetick applied to them with the same freedom as to finite real quantities, and suppositions of this nature multiplied, till the higher parts of geometry (as they were most commonly described) appeared full of mysteries.

“ FROM geometry the infinites and infinitesimals passed into philosophy, carrying with them the absurdity and perplexity that cannot fail to accompany them.”

AT page 46 Mr. Maclaurin says, “ We may perceive from these instances, that it is not by founding the higher geometry on the doctrine of infinites we can propose to avoid the apparent inconsistencies that have been objected to it;—.”

ACCORDINGLY, at page 49, it is said, “ Sir Isaac Newton accomplished what Cavalerius wished for, by inventing the method of fluxions, and proposing it in a way, that admits of strict demonstration *, which requires the supposition of no quantities but such as are finite, and easily conceived. The computations in this method are the same as in the method of infinitesimals; but it is founded on accurate principles, agreeable to the ancient geometry †. In it, the premises and conclusions are equally accurate, no quantities are rejected as infinitely small, and no part of a curve is supposed to coincide with a right line.”

FARTHER, Mr. Maclaurin has expressly copied Mr. Robins’s representation of fluxions in these words, Art. 10, page 56, 57. “ The velocity

* Discourse, §. 3, 94, 142, 150. Account, §. 4. Dissertation, §. 1. † Discourse, §. 3, 142, 150.

“ with

“ with which a line flows, is the same as that of
 “ a point which is supposed to describe or generate
 “ it *. The velocity with which a surface flows,
 “ is the same as the velocity of a given right line,
 “ that, by moving parallel to itself, is supposed to
 “ generate a rectangle which is always equal to the
 “ surface †. . . . In this method likewise quantities
 “ of the same kind may be represented by right
 “ lines, and the velocities of the motions by which
 “ they are supposed to be generated, by the ve-
 “ locities of points moving in right lines ‡.”

AND the sense of this paragraph is delivered
 by Mr. Maclaurin in the Philosophical Transactions,
 N^o 468, page 329 ; as follows.

“ LINES are supposed to be generated by the
 “ motion of points. The velocity of the point that
 “ describes the line is its fluxion, and measures the
 “ Rate of its Increase or Decrease ||. Other magni-
 “ tudes may be represented by lines that increase
 “ or decrease in the same Proportion with them;
 “ and their Fluxions will be in the same Proportion
 “ as the Fluxions of those lines, or the Velocities
 “ of the Points that describe them ‡||.”

THE uses to be made of these velocities, Mr.
 Maclaurin thus expresses, in conformity with Mr.
 Robins, in the last Article of his book.

“ In this doctrine, when the velocity of a motion
 “ is determined, it is always with relation to the
 “ velocity of some other motion; and when we
 “ enquire at what rate the ordinate, for example,
 “ increases or decreases, it is always in relation
 “ to the base, or some other magnitude, with which
 “ it is compared.” And thus Mr. Robins, “ It is
 “ by means of this proportion only, that fluxions
 “ are applied to geometrical uses; for this doctrine

* Discourse, §. 6. Dissertation, §. 3. † Discourse, §. 49.
 ‡ Ibid. §. 10, 11, 39. Dissertation, §. 7. || Discourse, §. 6.
 Dissertation, §. 3, 6. ‡|| Discourse, §. 10, 39. Dissertation, §. 7.

“ never

" never requires any determinate degree of velocity
" to be assigned for the fluxion of any one fluent*."

How Sir Isaac Newton applies his method of
prime and ultimate ratios to the quantitates nascentes
and evanescences, whence are demonstrated the pro-
portions of these velocities or fluxions, Mr. Maclaurin
in Article 502 thus declares. "... In order to
" avoid such suppositions" [infinitesimals] " Sir
" Isaac Newton considers the simultaneous incre-
" ments of the flowing quantities as finite, and then
" investigates the ratio which is the limit of the
" various proportions which those increments bear
" to each other, while he supposes them to decrease
" together till they vanish†; which ratio is the
" same with the ratio of the fluxions by what was
" shewn in Art. 66, 67 and 68‡. In order to
" discover this limit, he first determines the ratio
" of the increments in general, and reduces it to
" the most simple terms so as that (generally speak-
" ing) a part at least of each term may be inde-
" pendent of the value of the increments them-
" selves; then by supposing the increments to de-
" crease till they vanish, the limit readily appears."

CONCERNING this limit Mr. Maclaurin further adds
[Art. 505] that it is called the *first* or *prime ratio* of
these increments, and may with equal propriety be
said to be the *last* or *ultimate*; because though the
ratio of those increments continually varies, when
the motion is continually accelerated or retarded,
yet the ratio of the generating motion is the term or
limit from which the variable ratio of the increments
proceeds, or sets out to increase or decrease; and
soon after he observes, that a ratio may limit the
variable ratios of the increments, though it cannot
be said to be the ratio of any real increments||.

* Discourse, §. 15. † Account, §. 31. Review, §. 5.
Dissertation, §. 65. ‡ Discourse, §. 146, 149. Review, §. 5.
Dissertation, §. 65. || Discourse, §. 104, 105, 107, 108.
Account, §. 31. Dissertation, §. 58. 73. Now

Now this is the very doctrine Mr. Robins always insisted upon, and which Philalethes pretended to treat with so much scorn and contempt.

BUT to conclude, I ought not, perhaps, to omit, that Mr. Maclaurin has once indeed spoke of infinitesimals, which he has thus expressly condemned, more favourably than Mr. Robins has done, when he says [Introduction, p. 47] “ we would not be understood to affirm, that the method of indivisibles “ and infinitesimals, by which so many uncontested “ truths have been discovered, are without a foundation.”

WHAT this supposed foundation is, Mr. Maclaurin has no where explained ; and whether this was said by him in compliment to his friend Philalethes, or the effect of prejudices remaining from former opinions, I leave undecided. It is certainly inconsistent with all, that has been here cited from him.

THUS, upon the whole, I cannot doubt, but M. de Buffon, when he shall have calmly perused what Mr. Robins has writ, and observes, how exactly he has been followed by others ; he will see reason to wish, that all, he has said against my friend with so much insult, may be understood to proceed from his not having duly himself considered Mr. Robins's tracts, but taken upon trust the suggestions of some other, which were the effects of prejudice and ill will. And indeed when he talks of the obscurity of Mr. Robins's ideas, the insignificance of his phrases, and the unintelligibleness of his style ; he gives the most certain proof, that he had never carefully read his writings, or has a very imperfect knowledge of the language in which they are writ ; for Mr. Robins is much admired here for the contrary excellencies, on whatever subjects he has employed his pen.

M. de

M. de Buffon attempts to justify his abusive treatment of Mr. Robins, from his having the presumption to censure the Grand Bernoulli, as he styles him. This wonderful man, it seems, my friend has presumed to call an inelegant computist*; which assertion he was very able to justify from that writer's numerous productions. But the principal motive of his using this freedom was the indignation, he had early conceived at M. Bernoulli's insufferable vanity and indecent carriage, without any cause, towards many eminent mathematicians. In particular, Dr. Keill he had abused in a most outrageous manner; which scandalous behaviour the Doctor has treated, as it deserved, in his Letter to M. Bernoulli†, which Letter being in Latin, I wonder, it was not reprinted by the collectors of Dr. Keill's works: for though it has some personal reflexions, all justifiable from the usage he had received (a usage much blamed even by some of M. Bernoulli's best friends‡ and greatest admirers) yet it also contains curious particulars relating to the mathematicks; especially the applying second and third fluxions, in which many have committed errors. There the Doctor's assertions are supported by irrefragable arguments, the weakness of his antago-

* Remarks on Dr. Smith, §. 1. † J. Keill Epistola ad J. Bernoulli. Lond. 1720. in 4to.

‡ What an opinion M. de Montmort had of M. Bernoulli's behaviour, appears from his letters to Dr. Taylor. In one it is said, "Il est certain que M. Bernoulli est trop jaloux d'honneur et porte trop loin ses pretentions dans la dispute avec son frere, ils avoient tort tout deux mais luy à mon avis beaucoup plus que Jacques;" in another, "J'ay déjà pris la liberté de dire à M. Bernoulli que bien de gens trouvoient qu'il n'en usoit pas bien à l'égard de feu M. de l'Hospital;" and in a third, "Il y a un endroit contre M. Keill que je blame extrêmement—J'en suis fâché pour M. Bernoulli qui n'a pas été en droit de faire un pareil reproche à M. Keill quelque sujet qu'il puisse avoir de s'en plaindre." Keill Epist. ad J. Bernoulli, at the end.

nist's reasons clearly evinced, his abuses exposed, and the whole written in a very elegant manner. Inasmuch that I am apt to believe, if M. d'Alembert had duly examined this dispute, and seen this letter; he would scarce have said of M. Bernoulli, "Peut-être étoit-il excusable à l'égard de M. Keill, qui avoit en quelque maniere violé les règles du droit des gens, et dont les procédés n'étoient pas moins blâmables que les discours *."

AGAIN, my most dear and worthy friend Dr. Taylor, a gentleman of fortune; who allotted some part of his time to mathematical studies, and has published works of genius and invention; as his *Methodus Incrementorum* †, with several curious Dissertations in the *Philosophical Transactions* ‡. And observing the imperfect manner the art of Perspective was delivered in all the books he had seen, he wrote his treatise on that subject ||; which is highly esteemed by our most knowing painters ‡||. This excellent person M. Bernoulli fell upon without ceremony, to which attack the Doctor gave a sufficient answer in the *Philosophical Transactions*, N° 360;

* *Eloge Historique de M. Jean Bernoulli*, in M. d'Alembert's *Mélanges de Littérature*, &c. † An account of this book is given by the author himself in the *Philosophical Transactions*, N° 345. ‡ Ibid. N° 336, 352, 353, 354, and 367.

|| See an account of this Perspective by the author, *ibid.* N° 344.

‡|| As Dr. Taylor's treatise is written briefly, not to say obscurely; some have attempted to explain it. Though I have reason to think my friend found out by himself the excellent method, he described; yet he is not here an original inventor. For that very method was long before published by Guido Ubaldi, in his *Perspective* printed at Pesaro in 1600. Where it is delivered very clearly, and confirmed by most elegant demonstrations. In the last book, Ubaldi applies his method to the delineating the scenes of a theatre. And in this particular, with regard to the practice, he is followed by Signor Sabbatini in his *Practica di Fabricar Scene*; whereof was made, at Ravenna in 1638, a new edition, to which was added a second book, containing a description of the machines used for producing the sudden changes in the decorations of the stage.

setting

setting forth, at the same time, the gross errors his antagonist had committed in the solution of the Isoperimetrical problem. But M. Bernoulli persisting, that Dr. Taylor had stole from him the method of determining the center of oscillation, the Doctor demonstrated, that his own had been published in the Philosophical Transactions, N^o 337, before that of M. Bernoulli's *. On this clear conviction, M. Bernoulli had recourse to the low shift of pretending, that what Dr. Taylor had published in the Transactions, was but a sketch of what he afterwards produced in his *Methodus Incrementorum* †. Now in contradiction to this, it is certain, what was delivered in the Transactions, is more full and explicit, than what is contained in the *Methodus Incrementorum*, though even that treatise had been printed before M. Bernoulli's discourse was published.

LASTLY, in regard to Sir Isaac Newton, this great, modest, and generous person, has often, by M. Bernoulli, been most unreasonably insulted, and more particularly in a paper, that was published in an infamous libel, dated 29 Julii 1713 ‡, and there styled *Judicium primarii Mathematici*. That this primarius Mathematicus was M. Bernoulli, could not long remain a secret; as the paper bore such genuine marks of its real parent. However, M. Bernoulli three years after, in a letter to M. Leibnitz, wonders how it came to be known ||. But the weakness of this judgment has been fully exposed

* Aët. Erudit. Sept. 1722.
Tom. viii. Sect. v.

† Aët. Erudit. Supplem.

‡ This libel was translated into French, and printed at the Hague in the *Journal Litteraire* for Nov. and Decemb. 1713. p. 448.

|| “*Miror quomodo Newtonum scire potuerit me auctorem esse epistolæ illius, quam inseruisti chartæ illi contra Newtonum publicatæ; cum tamen nemo mortalium sciverit me illam scripsisse, nisi tu ad quem scripta est, et ego a quo scripta est.*” Leibnittii et Bernoullii *Commercium Epistolicum Lausanæ* 1745. Tom. ii. Epist. 231.

by Sir Isaac Newton himself, at the end of the *Commercium Epistolicum*. See also the *Journal Literaire* for July and August in 1714; where the gross abuses thrown on Sir Isaac Newton by the author of the libel itself, in relation to Dr. Hook and Mr. Flamstead, are excellently well answered by Dr. Keill. As to what was afterwards said in the *Acta Eruditorum* for March 1720, in giving an account of M. de Fontenelle's Elogium of M. Leibnitz, where an opportunity is taken of upbraiding Sir Isaac Newton for having been beholden to M. Huygens and Dr. Hook; it may be replied, that he had quoted them both in his *Principia*, Lib. i. Prop. iv. in the Scholium.

AGAIN, Mr. Robins's Remarks on a Treatise of M. Euler, are by M. de Buffon brought into the account. And here I cannot but observe the difference in the behaviour of a person of knowledge, who thinks for himself, and of one carried away with the unjust reports of envy and malice. M. Euler, highly to his honour as a gentleman, far from being influenced by any private disgust, not only has publicly praised, but has taken the pains to communicate to his countrymen in their own language, a work * of Mr. Robins's, which he thought worthy these marks of his esteem.

BUT to be more particular upon the railings thrown out against Mr. Robins in regard to Sir Isaac Newton.

M. de Buffon has charged Mr. Robins with want of respect towards that great man, by making him assert, that Sir Isaac Newton had not well considered the ancient geometers, that his Lemma was obscure and hypothetical, and his ideas not clear, and even setting himself above him.

* New Principles of Gunnery, translated into High Dutch by M. Euler. Printed at Berlin in 1745.

As M. de Buffon has not referred to any particular passages in Mr. Robins's writings; it is not easy to guess, where he pickt up all this string of accusations against one, who has always professed the greatest deference for Sir Isaac Newton*. Mr. Robins has indeed supposed, for which he had very good grounds, that, when Sir Isaac Newton invented his method of fluxions, he had not then much studied the ancients†. But is this asserting, that he had never well considered them?

NEITHER has Mr. Robins ever represented Sir Isaac Newton's Lemma as obscure, much less as in any measure hypothetical‡; nor has he any where insinuated, that Sir Isaac Newton was ever deficient in the perspicuity or distinctness of his ideas, declaring, in the strongest terms, that his express design was to shew, that the misapprehensions of others had given rise to all the objections, that had been advanced against Sir Isaac Newton's doctrine; which objections will be at once removed by a careful choice of expressions, adequately accommodated to Sir Isaac Newton's real meaning||.

MR. Robins has indeed made mention of the great brevity with which Sir Isaac Newton wrote‡||, a fact well known to every one, that reads him. If to declare this be a crime, he is followed in it by Mr. Maclaurin||†. But this was the most com-
plaisant

* Discourse, §. 129, 159. Account, §. 24. Dissertation, §. 93, 94, 96, 98. Remarks on Dr. Smith, §. 52. New Principles of Gunnery, in the preface, p. 51. † Dissertation, §. 93.

‡ Ibid. §. 42. || Ibid. §. 93. To the same purpose, it is said by M. d'Alembert, speaking of the differential method, " Il faut
" avouer que si ce calcul a eu des ennemis dans sa naissance,
" c'est la faute des géomètres ses partisans, dont les uns l'ont
" mal compris, les autres l'ont trop peu expliqué." Encyclo-
pédie, Tom. iv. p. 988. But here is the difference, it is im-
possible to justify M. Leibnitz's notions. ‡|| Discourse, §. 4.

Account, §. 28. ||† "What he [Sir Isaac Newton] has given
" on this subject being very short, his conciseness may be sup-

plaisant reason, Mr. Robins could alledge, why the author of the Analyst might not understand Sir Isaac Newton. It was in opposition to Dr. Berkeley, that Mr. Robins composed his treatise; all that he wrote afterwards against Philalethes, that gentleman brought upon himself by his impatience to see another enter the lists against his antagonist, with different weapons from his own; and turning his assault against him (as M. de Buffon expresses it) “comme défenseur de la vérité s’est chargé de lui signifier ce qu’on entendoit fort bien Newton sans Robins *;” but unfortunately for himself, as it now appears, that either he, or Mr. Maclaurin, as well as Mr. Robins, did not understand Sir Isaac Newton.

BUT if Mr. Robins had ascribed to Sir Isaac Newton some degree of negligence †, it will be no more than what was freely admitted by Dr. Saunderson, Professor of the Mathematicks at Cambridge, who, no body believes, ever wanted respect for Sir Isaac Newton; yet did own, “that the great Inventor, never expecting, to have it” [doctrine of fluxions] “canvassed with so much trifling subtilty and cavil, had not thought it necessary to be guarded every where by expressions so cautious, as he might have otherwise used ‡.”

THE truth is, Sir Isaac Newton at first made the same use of indivisibles, others had done: in his Analysis per æquationes numero terminorum infinitas, he expressly says, “Nec vereor lo- qui de unitate in punctis, five lineis infinite parvis ||;” and in his Lectiones Opticæ he

posed to have given some occasion to the objections which “have been raised against his method.” Maclaurin’s Treatise of Fluxions, p. 2.

* M. de Buffon’s preface, p. xxviii.

§. 101, 102.

† See Memoirs of Dr. Saunderson’s life, before his Algebra, p. xv.

|| Comm. Epist. p. 85.

‡ Dissertation, demon-

demonstrated by indivisibles *; where he also attempts, by their aid, to accommodate the definition of proportionality in commensurable quantities to such as are not commensurable †. But when he composed his Principia, he proposed to establish a more accurate method of conception; and explicitly instructs his readers, how they are to understand, whatever expressions may occur, that may appear similar to what are used in the doctrine of indivisibles, in saying, “ Siquando quantitates
“ tanquam ex particulis constantes consideravero,
“ vel si pro rectis usurpavero lineolas curvas;
“ nolim indivisibilia, sed evanescentia divisibilia,
“ non summas et rationes partium determinatarum,
“ sed summarum et rationum limites semper
“ intelligi ‡.”

BUT experience has shewn, that notwithstanding this caution, many had mistook Sir Isaac Newton's meaning, even after he had explained, in the Introduction to his Treatise on Quadratures, his doctrine in terms guarded against every objection, that could be raised.

AGAIN, M. de Buffon is too fond of his talent for invective to confine it to one object, but must needs extend it to Sir Isaac Newton himself also in the same heedless manner; that because Sir Isaac Newton died not long after the last publication of his Principia, he takes upon him to presume, that while that work was under his revision, his intellects were so impaired by age, that he was not master of his own thoughts, without considering in the least the cause of his death, which, notwithstanding his years, was not occasioned by the mere failure of

* Dissertation, §. 93. See also a demonstration of Sir Isaac Newton's, by indivisibles, in Hunt's Gauger's Magazine, printed at London in 1687.

† Lect. Optic. Part. 1. Sect. 3.

Lem. 6. Corol. 1.

‡ Princip. Lib. i. Lem. xi. in the Scholium.

the principles of life; but from one of the most tormenting diseases, that human nature is subject to *; for excepting some degree of that defect in memory in regard to recent occurrences, which is the usual concomitant of age, he is well known to have retained his intellectual faculties to the last. And this unjust reflection is thrown out upon Sir Isaac Newton only; because he had not pleased M. de Buffon in omitting a passage in his book relating to M. Leibnitz.

Now the original design of inserting this passage was to shew, that Sir Isaac Newton had before found out the method of fluxions. Of that affair he indeed had given an account in a letter written to Mr. Collins, of an earlier date † than that ‡ alluded to in the passage, whose omission is complained of by M. de Buffon. But for above 20 years after the Principia had been published, Sir Isaac Newton did not know, that this letter to Mr. Collins existed, or that a copy of it had been sent to Slusius, M. Tchurnhans, and M. Leibnitz. This letter contained a particular description of the method, he had delivered in the book of series and fluxions, which he had written the year before; and as he was prevailed on by Dr. Pemberton ||, while the last edition of his Principia was carrying on, to suffer this treatise of series and fluxions, after it should have been revised and augmented by himself, to come abroad; he thought proper to exchange the passage relating to M. Leibnitz, for what he had inserted from the abovementioned letter to Mr. Collins; for as it was written before M. Huygens's Horologium Oscillatorium was pub-

* See his elogium by M. de Fontenelle, in the History of the Royal Academy of Sciences at Paris for the year 1727.

† 10 Dec. 1672. ‡ 24 Octob. 1676. || View of Sir Isaac Newton's Philosophy, in the preface.

lished;

lished; so it mentioned Sir Isaac Newton's having then determined the curvature of curves *.

As to the passage, that regards M. Leibnitz, it may be observed, that Sir Isaac Newton has said, "He [M. Leibnitz] pretends, that in my Book of Principles, p. 253, 254, I allowed him the invention of the *calculus differentialis*, independently of my own †; and that to attribute this invention to myself, is contrary to my knowledge there avowed. But in the paragraph there referred unto, I do not find one word to this purpose; on the contrary, I there represent, that I sent notice of my method to M. Leibnitz before he sent notice of his method to me; and left him to make it appear that he had found his method before the date of my letter, that is eight months at least before the date of his own ‡." And afterwards Sir Isaac Newton adds, "As for the Scholium upon the second Lemma of the second Book of the Principia Philosophiæ Mathematica, which is so much wrested against me; it was written not to give away that Lemma to M. Leibnitz, but on the contrary to assert it to myself ||." And accordingly it is referred to and quoted at length in the dispute about the invention of fluxions **.

BUT Sir Isaac Newton finding M. Leibnitz, as well as others, had misrepresented this passage; he resolved to leave it out. On which account M. de Buffon asks, "Pourquoi supprimer cet article? " puisqu'on l'avoit laissé subsister dans la seconde

* Comm. Epistolic. p. 105. † Recueil de Diverses Pieces sur la Philosophie, &c. printed at Amsterdam in 1720, Tom. ii. p. 35, 42. ‡ Raphson. Hist. Fluxion. p. 115. As also in Recueil, &c. Tom. ii. p. 87. See also Philos. Trans. N° 342. p. 218; or Com. Epist. p. 51. || Raphson. Hist. Fluxion. p. 122. Or Recueil, &c. Tom. ii. p. 108. ** Journal Littéraire 1714. p. 340. Philos. Trans. N° 342. p. 198; or Comm. Epistolic. p. 30; and Recueil de Diverses Pieces, &c. Tom. i. in the preface, p. xiii.

“ edition en 1713. c'est-à-dire, dans le tems de la
 “ chaleur de la contestation.” I answer, that edi-
 tion was in the press, and great part printed off, be-
 fore the dispute about the invention of fluxions
 began, before Sir Isaac Newton knew, that his
 letter to Mr. Collins was in being, and long before
 the passage of the Principia had been misrepresented.
 And now there was no farther occasion for that
 passage; since Sir Isaac Newton's right to the inven-
 tion of fluxions had been fully demonstrated in the
Commercium Epistolicum. Besides, it did not be-
 come him to be upon any terms with such persons as
 M. Leibnitz and M. John Bernoulli, who not con-
 tent to usurp his inventions, had loaded him with
 calumnies. The former, not to insist on the inven-
 tion of fluxions, pretended to the discovery of the
 principal propositions of the Principia; but the
 paralogisms he committed, betrayed the plagiarism*.
 The latter, with no small degree of vanity, claimed
 to himself the integral calculus †, when what he had
 produced was but a very inconsiderable part of what
 is contained in Sir Isaac Newton's letters sent to
 M. Leibnitz ‡, and whoever shall consider the seven
 theorems, he long afterwards published || with the
 same degree of boasting, must be surprized at his
 obstinacy in persevering so to neglect, what Sir Isaac
 Newton has done, as not to know, that the four first
 of those theorems were but a small pittance of the
 series for the quadrature of curves, exhibited in the
 abovementioned letters; even after the use of it had
 been farther explained in the *Treatise of Quadratures*;
 nor that the three last were only one very simple case
 of the seventh proposition of that book, viz. when
 the part of the ordinate under the vinculum is a bi-

* *Philos. Transf.* N° 342. p. 208. Or *Commer. Epist.*
 p. 41, &c. *Journal Litteraire* 1714. p. 348. † *Acta*
Erudit. 1716. p. 299. ‡ *Keill Epist. ad Bernoul.* p. 3.
 || *Acta Erudit.* 1719. *Merf. Jun.* p. 269.

nomial,

nomial, and the index of that vinculum a negative integer. They talk indeed of their exponential calculus as a great discovery*; but this Sir Isaac Newton himself has hinted to be really of no use†.

THUS, I presume, I have sufficiently vindicated both my friend Mr. Robins and Sir Isaac Newton from the pragmatistical reflections of M. de Buffon. But as Sir Isaac Newton has at different times been censured on many points; I hope I may, without blame, here add, to what others have already writ in his justification, some animadversions on such objections, as I find to have been lately advanced or revived.

THE brevity, with which he has writ, particularly in his Principia, I observe still to be complained of‡; however M. de Buffon has been pleased to reproach Mr. Robins for making any concession on that head. But to such as have duly prepared themselves by a sufficient knowledge in geometry to enable them to understand his meaning (and who else ought to attempt it?) this conciseness is most agreeable§. In the beginning of his book, what relates to the laws of motion, and centripetal forces, is delivered most clearly; and therefore it ought not to have hindered any person of skill from comprehending, what he had demonstrated of the influence gravity has in the system of the universe. But his conciseness perhaps may have discouraged others less expert from giving

* "*Ut taceam calculi exponentialis, qui transcendentis perfectissimus, est gradus, quem L. . . . us primus exercuit, Johannes vero Bernoullius proprio Marte etiam assecutus est, nullam N. . . . o aut ejus discipulis notitiam fuisse.*" The paper dated 29th of July, 1713. Or Journ. Liter. 1713. p. 453.
 † Philof. Trans. N° 342. p. 212. Or Comm. Epist. p. 46. ‡ "Dans la plupart des endroits difficiles il [Newton] emploie un trop petit nombre de paroles à expliquer ses principes." Mem. de l'Acad. Royal. des Sciences à Paris 1745. p. 329. § "Prolixitas inutilis est obtusis et fastidiosa ingeniosis. Illis enim multa nil profuit; his vero pauca sufficiunt." Maurolyci, Opuscul. Mathem. p. 48.

due attention to what, he has delivered very distinctly; if they had, his doctrine of the extensiveness of the power of gravity could never have been conceived to be an attempt to establish that operation as an original power in nature, for which no cause was to be inquired after; and putting it upon the foot of the ancient principle of occult qualities. Certainly, when Galileo and Torricellius shewed the line, that projectiles would move in by the action of gravity upon them (so far as they were not resisted by the medium they passed through) they were never charged with introducing, under the name of gravity, an occult quality; nor will any one object to the explaining any of the effects of the spring of the air by those, who ascribed them to that elastic power, which the air is known to be possessed of, as asserting an occult quality, and reject those explanations, as unphilosophical, though the cause of that elasticity was, and still remains, wholly unknown to us. If the effects of no power in nature were to be inquired after, till that power could be traced up to its first original cause, all natural philosophy would be at an end.

Now no writer was ever more cautious, than Sir Isaac Newton has been, to avoid all objections of this kind. In the former part of his book, where he treats of the effects of centripetal powers in general, and of the tendency bodies may be supposed to have towards each other; he frequently repeats in the most explicit manner possible, that under the name of attraction he had no intention of confining it to any particular physical cause whatever *.

AND

* “ ———considerando vires centripetas tanquam attractiones,
 “ quarum fortasse, si physice loquamur, verius dicantur impul-
 “ sus.” Newton. Princip. Lib. i. Sect. xi. at the beginning. Again,
 “ Vocem attractionis hic generaliter usurpo pro corporum conatu
 “ quocunque accedendi ad invicem: sive conatus iste fiat ab
 “ actione corporum, vel se mutuo petentium, vel per spiritus
 “ emissos

AND when in the third book he applies the mathematical speculations of the first to the system of the world, he does not give the name of gravity to the centripetal powers, he finds to operate on the moon and other planets; till he has shewn, that action in the moon to be the same as that, which here upon the earth we call gravity. And from that time he substitutes this gravity in the room of the term attraction, he had before made use of*. In all this there is not the least ground to imagine, that he had any other idea of gravity, than what he had before expressed of attraction as a cause, whose effects he treats of without taking upon him to determine, whence that power arose.

I AM as much surprized on the other hand, that what, Sir Isaac Newton has suggested at the conclusion of the latter editions of his book concerning the possibility of the operation of gravity being, owing to some subtle fluid extended through the universe, should be considered as drawn from him by these objections, I have been speaking of, against the general tenour of his former sentiments†; when in one of the passages ‡ just now cited, which occurs

“*emissos se invicem agitantium; sive is ab actione ætheris, aut aeris, mediæ cujuscunque seu corporei seu incorporei oriatur corpora innatantia in se invicem utcunque impellentis.*” Ibid. Lib. i. at the end of Sect. xi. in the Scholium. See also Philos. Trans. N^o 342. p. 222, 223. Or Com. Epist. p. 55, 59.

* “*Propterea vis illa, qua luna retinetur in orbe suo ea ipsa erit quam nos gravitatem dicere solemus.*” Newton. Princip. in Schol. Prop. iv. Lib. iii. Again, “*Hactenus vim illam qua corpora cœlestia in orbibus suis retinentur centripetam appellavimus. Eandem jam gravitatem esse constat, et propterea gravitatem in posterum vocabimus.*” Ibid. in Schol. Prop. v. Lib. iii.

† “*Le principe d’une matiere subtile—que M. Newton lui-même, plus intéressé que personne à ne le pas admettre, n’a pu s’empêcher d’adopter—*” Hist. de l’Acad. Royal des Sciences à Paris 1749. p. 53.

‡ “*Sive conatus iste fiat ab actione ætheris*” &c. Princip. Lib. i. Sect. xi. at the end.

in

in the first edition as well as in the following, he has so explicitly enumerated this among the other causes, he has suggested as possibly capable of producing the effect.

HOWEVER, as effects and causes cannot be extended in infinitum, there must be some first cause, to which the subsequent are owing. But that Sir Isaac Newton was not so hasty as to conclude gravity to be such a first cause, is evident, from what has been here said. Though I find M. d'Alembert rather supposing that he considered it as such; confirming himself in that opinion from Mr. Cotes having adopted that sentiment in a preface, which M. d'Alembert presumes to have been writ under Sir Isaac Newton's inspection, and to have received his entire approbation*.

Now Sir Isaac Newton has himself informed us †, “*Quæ novæ Principiorum editioni præmissa sunt, Newtonus non vidit antequam Liber in lucem prodiret;*” and it is well known, that he was much dissatisfied with that preface for more reasons than one; which contains things unworthy of him, in particular towards the end, where mention is made of Dr. Bentley; and that he therefore once intended to have made another edition of his book, in order to omit that preface, being much displeased with Dr. Bentley on that account. However, his mild temper could not long retain resentment against one, with whom he had for a great part of his life a familiar acquaintance; so that when at length he came to make the last edition of his *Principia*;

*—“ il [Newton] a souffert que M. Cotes son disciple “ adoptât ce sentiment sans aucune réserve, dans la préface qu’il “ a mise à la tête de la seconde édition des *Principes*; préface “ faite sous les yeux de l’auteur, et qu’il paroît avoir ap- “ prouvée.” *Encyclop. Tom. i. p. 854.* The same thing is repeated in another place of the *Encyclopédie*, *Tom. vii. p. 876.* † *Comm. Epistolic. in the preface, pag. penult.*

in regard to the memory of Mr. Cotes, and an unwillingness to offend Dr. Bentley, who was then alive, and to whom he had been for many years reconciled, he suffered that preface to be again prefixt to his book.

THOUGH this preface is one, and even the most considerable of those writings, which gave occasion to what he was wont to complain of, That opinions, which he never had entertained, have been imputed to him; because they were found in the writings of others, whom some had been pleased to call his disciples. But that no opinions may be attributed to this great man, which he never held; see what he has said excellently well himself in the Philosophical Transactions, N^o 342. p. 222 *, about his method of philosophising. See also his letter to Signor Conti †.

HERE M. d'Alembert intended to concur with Sir Isaac Newton; though he has been mistaken in ascribing to him an opinion, he never held. In another place ‡ he joins with the Bernoulli's, when they charged Sir Isaac Newton with having committed a mistake in determining second fluxions ||. And though that affair had been already cleared up from their objections by Sir Isaac Newton himself in the Philosophical Transactions, N^o 342. p. 209 ‡||; yet M. d'Alembert has again renewed the accusation, which he supports by insisting, that Sir Isaac Newton had confounded the idea of a real curve with an imaginary form, which should consist

* Com. Epist. p. 55, &c. † Raphs. Hist. Fluxion. p. 100; or Recueil des Pieces, &c. Tom. ii. p. 16. ‡ Encyclop. Tom. iv. p. 988. § Mem. de l'Acad. Royal des Sciences Ann. 1711. ‡|| Or Com. Epistol. p. 40. See also what Dr. Keill has said in the Journal Litteraire in 1714. p. 343; and in 1716. p. 428; and in his letter to Bernoulli, p. 16. See likewise Riccati's letter, at p. 24. of Suzius's Disquisitiones Mathematicæ, printed at Venice 1725. In so many places has this affair been settled.

of polygons *. Whereas the fact is, those, who wrote upon curves according to the principles of indivisibles, were guilty of confounding these two ideas together; and Sir Isaac expressly distinguished them. Those, who had considered curves as consisting of polygons of an infinite number of sides, supposed the tangent to be one of these sides continued; whereby the portion, intercepted between the curve and tangent in an ordinate parallel to that through the point of contact, has for its ultimate limit the second fluxion of that ordinate; but Sir Isaac Newton has considered the tangent not as coinciding with any particle of the curve; but as making angles with every line drawn from the point of contact within the curve either way; and thus the ultimate limit of the aforesaid portion, intercepted between the curve and tangent, is half only of what was assigned by others.

SIR Isaac Newton seems also to be blamed by M. d'Alembert in the *Encyclopédie* † for having used synthetick demonstrations; as if he did it to conceal his method of investigation. But Sir Isaac Newton has himself told his real motives ‡; and so long ago as 1671 he advised, after a problem is once solved by algebra, to demonstrate the solution; “that laying aside all algebraical calculations, as much as may be, the theorem may be adorned, and made elegant, so as to become fit for publick view ||.” And in his algebra he has blamed the introducing the expressions of arithmetick into geometry. He there says, “Æquationes sunt expressiones computi arithmetici, et in

* “Il (Newton) a confondu la courbe polygone avec la rigoureuse.” *Encyclop.* Ibid. † *Tom. vii. p. 637.*

‡ *Philos. Transf. N° 342. p. 206; or Comm. Epist. p. 39.*

|| *Method of series and fluxions, Prob. ix. §. 107.* I have above quoted the words of the English translation; because the French translation gives not the full force of the passage.

“ geo-

“ geometria locum non proprie habent.—Multi-
 “ plicationes, divisiones, et ejusmodi computa in
 “ geometriam recens introducta sunt; idque incon-
 “ sulto, et contra primum institutum scientiæ hujus.
 “ —Proinde hæ duæ scientiæ confundi non debent.
 “ Veteres tam sedulo distinguebant eas ab invicem,
 “ ut in geometriam terminos arithmeticos nun-
 “ quam introduxerint. Et recentes utramque con-
 “ fundendo amiserunt simplicitatem in qua geo-
 “ metriæ elegancia omnis consistit *.”

AND other writers before him had spoke to the same purpose †. But his example, M. d'Alembert is of opinion, has had an ill influence on the English mathematicians, who hence becoming great espousers of the synthesis, have not gone such lengths in geometry, as might otherwise have been expected from them; and after enumerating many improvements in the physical astronomy, adds, “ Qu'on essaye d'employer la synthese à ces recherches, on sentira combien elle est incapable.”

HERE this ingenious gentleman confounds the analysis and synthesis together; an inadvertency, he might have avoided, had he recollected, what he

* *Arithmetica Universalis*, p. 288. at London in 1722.

† “ Neque vero placet barbarum idioma, id est algebricum; geometrica geometrice tracto.” *Vietæ Op.* p. 305. “ Artifex geometra quanquam analyticum edoctus, illud desimulat et tanquam de opere efficiundo cogitans profert suum syntheticum problema, et explicat.” *Ibid.* p. 335. “ Solutio non est geometrica, siquidem algebra geometrica non est—.” *Kepler de Motu Martis*, p. 93. “ Monemus tantum viros clarissimos [Wallis et alios] ut sepositis tantisper speciebus analysis problemata geometrica viâ Euclidianâ et Apollonianâ exequantur, ne pereat paulatim elegancia et construendi et demonstrandi, cui præcipue operam dedisse veteres innuunt satis et data Euclidis et alii a Pappo enumerati analyseos libri.” *Fermat's Epistle to Sir Kenelm Digby*, first printed in *Dr. Wallis's Commercial Epistolicum* in 1658, and afterwards reprinted in Vol. II. of his works in 1693. This letter is not in *M. Fermat's Mathematical Works* printed at Thoulouse in 1679.

has said in another place *, where he acknowledges the ancients to have had an analysis of their own different from ours; he could not but have seen, how insufficient the terms of synthesis and analysis are to distinguish the ancient geometrical analysis from the modern algebra.

ANALYSIS is in general the unravelling the state of a question, and conducting it back to some known principles; and this may either be effected by the ancient method, which extends itself through the whole compass of geometry in search of the nearest and most appropriate principles; or by the modern computations of algebra, which are confined to a very few elementary propositions only †. And in like manner the solution of any problem may be proved after the ancient method of synthesis or composition from such propositions already known, as will most directly lead to the point in question; or by the round-about synthesis of an algebraical calculation, composing the proof from the most remote principles, in compliment to such, as have learnt only two or three elementary propositions. This algebraical synthesis abounds in the modern writers ‡; but such as were apprized of the excellency of the ancient demonstrations, whatever use they might have made of the algebraic analysis, thought it a necessary improvement of their solutions, to prove them by a synthesis of a more elegant form.

AND here, it may be remarkt, that an observation made by M. d'Alembert, on the analysis of the ancients, requires correction; who says, " nous ignorons en

* Encyclop. Tom. i. p. 401. † See a letter of Des Cartes to the Princess Elizabeth, being the 80th of the 3d Vol.

‡ See an eminent instance at p. 331. of the 1st Vol. of Cartes's Geometry, where Schooten endeavours to make out by algebra in a dozen pages, what James Gregory demonstrated in half that number of lines in his *Geometriae pars Universalis*, p. 130.
" quoi

"*quod consistit precipue in analyſe **;" when Pappus has given ſo diſtinct and particular a relation of it with many examples. And it has been formerly practiſed and deſcribed by divers of the modern geometers †; but perhaps no where more diſtinctly explained than in the Scholia on Vieta's *Iſagoge*, printed at Paris in 1631; which particular ſcholia were omitted by Schooten, when he publiſhed Vieta's works. Joannes della Faille promiſed to write upon it ‡; and Hugo de Omerique in his *Analyſis Geometrica*, printed at Cadiz in 1698, treats of it, though as a diſcovery of his own. I obſerved above, that the perfecteſt knowledge of it is to be learnt from Dr. Halley's edition of Apollonius de *Sectione rationis*.

BUT the practice of this analyſis requires more knowledge in geometry, than M. d'Alembert ſeems to think neceſſary; conceiving that much reading in the mathematicks may hinder making diſcoveries, by ſo filling the head with foreign ideas, as to leave no room for the admiſſion of new ones of our own ||. This fancy is confuted by the examples of Vieta, Fermat, Huygens, Sluſius, Barrow, and James Gregory, who, though they were great eſpouſers of the ſyntheſis, were juſtly eſteemed the principal improvers of the modern geometry; and their thorough acquaintance with the writings of their predeceſſors, I preſume, laid no reſtraint on their own invention: if his Hero Bernoulli had

* *Encyclopédie*, Tom. i. p. 677. † "At algebra quam tradidere Theon, Apollonius, Pappus et alii veteres analyſtæ geometrica eſt." Vieta Op. p. 339. "Subjiciam igitur aliquot problemata, quæ ſub algebra non cadunt, ea quæ reſolvam et componam methodo quæ veteres in reſolvendis et componendis problematibus utebantur." Ghetaldus de *Reſolutione et Compositione Mathematica*, printed at Rome in 1630. p. 330. I mentioned M. Fermat's authority above. ‡ In the preface to his book *De Centro Gravitatis circuli*, &c. || *Encyclopédie*, Tom. i. p. 401.

imitated them, he would have saved himself five years puzzling about so easy a problem, as the determining the time of the shortest twilight *; seeing it already done with great elegance by Petrus Nonius †. And if he had duly considered the works of Sir Isaac Newton, and others, and not depended so much on his own uninformed invention; he might, by gaining of time, and acquiring a better manner of writing, have made still further improvements, and given a more elegant turn to his investigations.

I COME lastly to give some account of Sir Isaac Newton's discoveries; not forgetting his defence, as occasion shall offer.

M. de Buffon, at the beginning of his preface, has said, "L'ouvrage dont on donne ici la Traduction a été commencé en 1664 et achevé en 1671." But this is not exact; for Sir Isaac Newton composed it in a very short space of time; just before Christmas in 1671. He never finished much above half of what he designed ‡. The resolution he had taken towards the end of his life, of revising and augmenting it, being prevented by his death; it came into the hands of his heirs, in the state it was originally written. They indeed would have put it forth, in the condition it was left; had not the bookseller declined the terms, upon which it was offered him.

WE know from Sir Isaac Newton himself ||, that he was first led to his method of exhibiting by infinite series the areas of curves not quadrable in finite terms, from considering the method Dr. Wallis proposed in his Arithmetick of infinites.

* Eloge Historique de M. Jean Bernoulli, in M. d'Alembert's *Mélanges de Littérature*, &c. † Nonius de *Crepusculis*, printed at Lisbon in 1542. ‡ General Dictionary, under the article Newton. See also *Com. Epist.* p. 165. || *Com. Epist.* p. 159.

THE DOCTOR for this purpose considered the subject thus. That if the ordinate of any curve consist of two terms, one of which was an invariable quantity, and the other some power of the absciss; he shews, not only, how this curve, but others also, whose ordinates should be the several integral powers of this in succession, were to be measured; and then takes into consideration, how the areas might be expressed of curves consisting of fractional powers, which should be inserted intermediately between each of the terms of this series with integral powers, so as to compose a series of this form, $1, 1 - \sqrt[3]{xx}, 1 - \sqrt[4]{xx}, 1 - \sqrt[5]{xx}, 1 - \sqrt[6]{xx}, 1 - \sqrt[7]{xx}, \&c.$ For these Dr. Wallis found an infinite series to be necessary; and discovered such a series upon this principle for the mensuration of the circle. Sir Isaac Newton, in considering this method, observed, that by what is now usually called his Binomial theorem, the several terms, which compose the areas of the original series of quadrable curves, might be assigned: and concluded, that the same might be applied to the intermediate curves, with this difference, that in the curves, whose ordinates were an integral power of the first, the series would exhibit a certain finite number of terms; but when applied to the intermediate terms with fractional powers, the series would run on in infinitum. And this gave the first rise to this celebrated theorem.

BUT soon after he observed, that this theorem might more commodiously be applied to express those intermediate ordinates themselves; and then, in searching after a more direct proof of the truth of this theorem, he found, he could extend the methods of division and extraction of roots, taught in books of algebra, into the very same series; and appears to be the first, who thought of continuing on those operations in this manner: Dr.

Wallis observes of himself, that though he was not unapprised of those operations [in his treatise of Arithmetick he had described them *] yet this continuation of them had escaped him †.

THESE discoveries Sir Isaac Newton made in a short space of time, between the years 1664 and 1665. Then also he found out a method of tangents, like that Slufius afterwards produced, and how to determine the curvature of curves ‡.

IN the summer of 1665, besides employing himself in making trials upon the facility, wherewith his series for measuring hyperbolick spaces might be applied to the computing logarithms to a great number of figures ||; he first thought of his method of expressing universally the 59th proposition of Dr. Wallis's Arithmetick of infinites by an indefinite index.

THESE discoveries, with others, were communicated to M. Leibnitz in 1676, by Sir Isaac Newton's letters. In them was the binomial theorem, where it was shewn, how by it to raise any nomial whatever to any power, and to extract its root ‡||; there also M. Leibnitz was instructed in the use of indefinite indexes; yet it is customary to attribute

* Cap. xx. reprinted in the 1st Vol. of his Op. Mathematica.

† "Non quod ego nesciverim divisionem et radicum extractionem posse in speciebus institui: sed quod in hujusmodi casibus res abitura sit in infinitum, pariter ac si velim fractionem $\frac{1}{2}$ ad decimalem redigere 0.3333 &c. aut numeri non quadrati 2 radicem quadraticam exquirere 1.4142 &c." Wallisii Op. Math. Vol. II. p. 376. ‡ General Diction. Art. Newton: Concerning this curious speculation it may be observed, that Apollonius had in his 5th book determined the point, where only one perpendicular could be drawn to the conic-section; and this point is the center of the circle, which is now considered as of equal curvature with the conic-section at the point, where the perpendicular falls: and Kepler [p. 76. in his Paralipomena to Vitellio] makes express mention of these circles in the parabola, and observes them to be different in different parts of the curve. || Comm. Epist. p. 164. ‡|| Ibid. p. 132. their

their introduction into analysis also to that gentleman, even prior to Sir Isaac Newton *.

As to the invention of series, there has been raised some doubts. It is said, Sir Isaac Newton himself has acknowledged, that he found them out at first by very indirect means †.

How he discovered and compleated that invention has been above related; all which he accomplished, three years before Lord Brouncker and Mr. Mercator published their quadratures of the hyperbola by a series ‡. The former indeed had found out that series long before; for it is mentioned by Dr. Wallis in 1656 ||; but such a series is not a general method. As to Mr. Mercator, he lived 16 years after, and made no farther progress or pretensions ¶. He could not but know, what Sir Isaac Newton had done, either from Mr. Collins, or at least from Sir Edward Sherburne's Translation of the Sphere of Manilius (which he had read ||†), where, in the Appendix p. 116, Sir Isaac Newton's inventions are mentioned; and indeed Mr. Mercator kept a correspondence ** with Sir Isaac Newton himself; and in his excellent Astronomy, printed in 1676, at p. 286, speaking of the librations of the moon, he says, "*Harum tam variarum atque implicitarum librationum causas hypothesi elegantissimâ explicavit nobis Vir cl. Isaac Newton, cujus humanitati hoc et aliis nominibus plurimum debere me lubens profiteor.*" Sir Isaac Newton's additions to Mercator's Astronomy were mentioned in the Appendix to Sherburne's Manilius, printed in 1675.

* Dict. Universelle de Mathematique et de Physique par M. Saverien à Paris 1753. Tom. i. p. 361. §. 5. † Ibid. p. 423. ‡ Com. Epist. p. 95, 103, 130; and also General Dict. Art. Newton. || Oper. Math. Tom. i. p. 232. ¶ Philof. Transf. N^o 342. p. 174. Or Com. Epist. p. 3. ||† See the Preface to his Astronomy. ** Princip. Prop. xvii. Lib. iii. of the last edition.

IN the year 1665, Sir Isaac Newton also found out the direct method of fluxions, and the following year the inverse method, as likewise the theory of light and colours, and considered the effects of gravity on the moon, and on the planets gravitating towards the sun *.

BUT as to the invention of fluxions, Sir Isaac Newton himself, in answer to what M. Leibnitz had boasted of his own discoveries, has said, " And am
" not I as good a witness that I invented the
" methods of series and fluxions in the year 1665,
" and improved them in the year 1666, and that I still
" have in my custody several mathematical papers,
" written in the years 1664, 1665, and 1666, some
" of which happened to be dated; that in one of
" them, dated the 13th of Novemb. 1665, the direct
" method of fluxions is set down in these words.

" PROB. An equation being given, expressing
" the relation of two or more lines x , y , z &c.
" described in the same time by two or more
" moving bodies A, B, C &c. to find the relation
" of their velocities p , q , r &c.

" RESOLUTION. SET all the terms on one side of
" the equation, that they become equal to nothing.
" Multiply each term by so many times $\frac{p}{x}$ as
" x has dimensions in that term. Secondly, Mul-
" tiply each term by so many times $\frac{q}{y}$ as y hath
" dimensions in it. Thirdly, Multiply each term
" by so many times $\frac{r}{z}$ as z hath dimensions in it,
" &c. The sum of all these products shall be
" equal to nothing. Which equation gives the

* " Aussi grand au moins par ses experiences d'optique que
" par son système du monde," says M. d'Alembert, speaking of
" Sir Isaac Newton in describing his philosophy. Encyclop.
" Tom. vi. p. 298.

" relation of p, q, r &c. And that this resolution
 " is there illustrated with examples and demon-
 " strated, and applied to problems about tangents,
 " and the curvature of curves. And that in an-
 " other paper, dated the 16th of May 1666, a
 " general method of resolving problems by motion
 " is set down in seven propositions, the last of
 " which is the same with the problem contained
 " in the aforesaid paper of the 13th of Novemb.
 " 1665. And that in a small tract written in
 " Novemb. 1666 the same seven propositions are
 " set down again, and that the seventh is improved
 " by shewing how to proceed without sticking at
 " fractions or surds, or such quantities as are
 " now called *Transcendent*. And that an eighth
 " proposition is here added, containing the inverse
 " method of fluxions so far as I had then attained
 " it, namely, by quadratures of curvilinear fi-
 " gures, and particularly by the three rules upon
 " which the *Analysis per æquationes numero termi-*
 " *norum infinitas* is founded, and by most of the
 " theorems set down in the *scholium* to the tenth
 " proposition of the book of quadratures. And
 " that in this tract, when the area arising from
 " any of the terms in the valor of the ordinate
 " cannot be expressed by vulgar analysis, I repre-
 " sent it by prefixing the symbol $\square$ to the term.
 " As if the *abscissa* be x , and the ordinate $ax -$
 " $b + \frac{bb}{a+x}$, the area will be $\frac{1}{2}axx - bx + \square$
 " $\frac{bb}{a+x}$. And that in the same tract I sometimes
 " used a letter with one prick for quantities in-
 " volving first fluxions; and the same letter
 " with two pricks for quantities involving second
 " fluxions *. And that a larger tract which I

* In his problem for determining the curvature of curves.

" wrote

“ wrote in the year 1671, and mentioned in my
 “ letter of the 24th of Octob. 1676 *, was founded
 “ upon this smaller tract, and began with the
 “ reduction of finite quantities to converging
 “ series; and with the solution of these two pro-
 “ blems: 1. *Relatione quantitatum fluentium inter*
 “ *se data, fluxionum relationem determinare.* 2. *Ex-*
 “ *posita æquatione fluxiones quantitatum involvente,*
 “ *invenire relationem quantitatum inter se.* And
 “ that when I wrote this tract, I had made my
 “ analysis composed of the methods of series and
 “ fluxions together, so universal, as to reach to
 “ almost all sorts of problems, as I mentioned in
 “ my letter of the 13th of June 1676 †, and this
 “ is the method described in my letter ‡ of the
 “ 10th of December 1672 ||.”

ABOUT the year 1708, all the papers of Mr. John Collins, who had kept a correspondence with the most eminent geometers in Europe, fell into the possession of Mr. William Jones, then a teacher of the mathematicks in this city. Amongst them were copies of some treatises of Sir Isaac Newton, particularly of his *Analysis per æquationes numero terminorum infinitas* ‡||. These Mr. Jones communicated to their author, who thereupon lent him the abovementioned tracts, which Mr. Jones transcribed, and used to distribute fragments of them to his scholars. So that many such are in the hands of different persons.

AMONGST them was a small tract divided into two parts. In the first leaf was written “ *Con-*
 “ *structiones Geometricæ Æquationum per D. Isaac*
 “ *Newtonum. Ex Apographo Dⁿⁱ Collins.*”

THE first part is a sort of Elements of geometry,

* *Commerc. Epistolic.* p. 165.

† *Ibid.* p. 141.

‡ *Ibid.* p. 105.

|| *Raphf. Hist. Fluxion.* p. 115. Or

Recueil &c. Vol. II. p. 87.

‡|| *Phil. Transf.* N^o 342.

p. 176. Or *Com. Epist.* p. 5.

where

where motion is introduced, in order to effect several problems after a different manner from what is common, and there is given constructions of solid problems (some of which are transcribed into his Algebra) by placing a streight line of a given length between two other lines given in position, so as to pass through a given point, &c. and from the facility, wherewith this may be performed mechanically, by moving the given line between the others, till it arrives at the position required; he proposes to admit into geometry such motions, as an additional postulate to those of the elements.

THE second part, which answered to the title, somebody had prefixt to the whole, delivered in eight problems the constructions of quadratic, cubic, bi-quadratic, and equations of higher powers by the circle, conic-sections, and cubical parabola. The 9th and last problem was this, "*Quomodo problemata solvenda sunt, ubi per intricatam terminorum complicationem non licet ad æquationes commode pervenire;*" but the solution was wanting. And indeed this copy was very imperfect; as I learn from some passages extracted from another manuscript. The construction by the cubical parabola * Sir Isaac Newton speaks of in a letter to Mr. Collins, dated July 13, 1672 †.

As some of these constructions are particularly accommodated for obtaining the first figures of the roots of equations ‡; so Sir Isaac Newton found out two other expedients for the same purpose. The first of these he takes notice of in the abovementioned letter to Mr. Collins, and it was described by Mr. Oldenburgh in a letter to M. Leibnitz, dated the 24th of June 1675 §. But I shall here give a particular account of both. If

* Enumeratio lin. tertii ordin. Sect. vii.

† General

Dictionary Art. Newton, p. 782.

‡ Newton Arith.

Univerf. p. 285.

§ "*Dominus Newtonus, beneficio logarithmorum graduatorum in scalis $\omega\alpha\pi\alpha\lambda\lambda\acute{\alpha}\nu\varsigma$ locandis ad*

" distantias

If within a frame several equal lines of logarithmetick numbers, such as are described on Gunter's



scale, AB, CD, EF and GH are placed equidistant from one another, and all but the last GH upon

separate moveable sliders, and upon the fixt side of the frame a point P be placed at the same distance from the first of those lines. Then, when the beginning of all these lines stand directly under the point P, a ruler laid from P to any number upon the first line AB shall mark upon the second line CD the square of that number, upon the line EF the cube, and upon GH the fourth power of the same. But if any one of the sliders be moved backwards, the ruler shall mark upon that slider the respective power of the forementioned number multiplied by the number, which in this slider in its present situation stands directly under the point P.

THEREFORE any equation $z^4 - az^3 - bz^2 + cz = m$ being given, if the slider EF be moved back, till in that slider the number a stand directly under the point P, and if the number b in the slider CD, and the number c in the slider AB, be likewise brought under P, then, if the ruler laid from P mark upon GH the true value of z^4 , it will mark upon EF the value of az^3 , upon CD the value of bz^2 , and upon AB the value of cz , and these collected together

"distantias æquales, vel circulorum concentricorum eo modo
 "graduatorum adminiculo, invenit radices æquationum. Tres
 "regulæ rem conficiunt pro cubicis, quatuor pro biquadraticis.
 "In harum dispositione respectivæ coefficientes omnes jacent in
 "eadem linea recta; a cujus puncto tam remoto a prima regula ac scalæ graduatæ sunt ab invicem, linea recta iis super-
 "extenditur, una cum præscriptis conformibus genio æquationis,
 "qua in regularum una datur potestas pura radicis quæsitæ."
 Comm. Epist. p. 123.

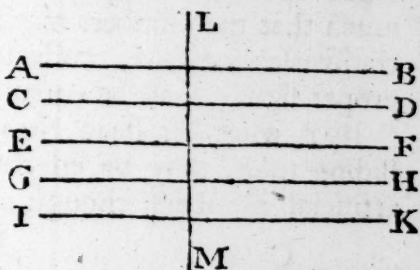
under

under their proper signs will be equal to m ; but when the numbers markt out by any situation of this ruler, being thus collected together, are either greater or less than m , then the ruler does not mark upon the line GH any true value of z^4 , but by moving its situation, and collecting the numbers markt upon the sliders, all the values of z^4 , and thence all the roots of the equation, may by repeated trials be found.

His second method, which is more commodious, was thus.

If within a frame several lines of numbers are placed after this manner. Suppose the first AB fixt upon the side of the frame, but the rest

upon parallel sliders, the first of which CD shall be the same with AB, the second EF shall have its divisions but half as long, the



divisions of the third GH one third, and the divisions of the fourth IK one fourth, of the divisions of the first; then, when the beginning of these lines stand under each other, if any line LM be drawn perpendicularly cross them, it shall mark upon the first of the sliders the same number as on the fixt line, upon the second slider the square of that number, upon the third the cube, and upon the fourth the fourth power of the same. But if any one of these sliders be moved backwards, then shall the transverse line LM mark upon that slider the respective power of the forementioned number multiplied by the number, which in this slider, in its present situation, stands directly under the point A, the beginning of the fixt line.

Now let any equation be given $az + bz^2 - cz^3 + dz^4 = m$. If the slider CD be moved back, till
in

in that slider the number a stand directly under A, and if the number b in the slider EF, and the



number c in the slider GH, and the number d in the slider IK, are also brought under A; then, if the transverse line

LM mark upon the line AB the true value of z , the root of the equation, it will give upon CD the value of az , upon EF the value of bz^2 , upon GH the value of cz^3 , and upon IK the value of dz^4 ; inso-much that the numbers markt upon the sliders by the transverse line LM, collected together under their proper signs, shall be equal to m .

BUT what Sir Isaac Newton had done by these sliding rules, may be effected by any single line of artificial numbers, though not so expeditiously; for

if AB be such a line, in which the number expressing the value of the root z of the foregoing equation be at C, the distance AC taken from the number a will give upon this line az , and double this distance taken from b will give bz^2 , and triple the same distance taken from c will give cz^3 , and four times the distance taken from d will give dz^4 . Which indeed may be performed most commodiously by these numbers inscribed on a circle after Oughtred's manner*.

THERE were also papers containing problems relating to the center of gravity; and to curves intersecting right lines given in position in given angles; and several examples of finding fluents from fluxions according to what is said at the end of the quadratures, "*Fluens pro lubitu assumi potest, et assumptio*

* See his Treatise, called *The Circles of Proportion*, &c. printed at London in 1633.

“ corrigi, ponendo fluxionem fluentis assumptæ
 “ æqualem fluxioni propositæ, et terminos homo-
 “ logos inter se comparando.”

THERE was one paper intituled, Notæ in Aët. Erudit. An. 1689, 1706. This ended thus, “ Prop. 19, col-
 “ ligitur ex duabus, falsis propositionibus 12^a et 15^a.
 “ Nam 12^a non valet nisi in circulo viribus concen-
 “ trico, et prop. 15. falsissima est. Leibnitzius
 “ igitur non invenit prop. 19. per calculum differen-
 “ tialem, sed inventam computare conatus est, ut
 “ suam faceret, et computando bis erravit *.” But
 these paralogisms of M. Leibnitz have been quoted
 even as preferable to Sir Isaac Newton’s most clear
 and accurate demonstrations. After M. Leibnitz had
 been shewn these paralogisms, he was far from ac-
 knowledging his mistakes, saying, “ Paralogismus,
 “ quem mihi Keilius imputat, nihil est, et redit ad
 “ *modum loquendi*.” Epist. Leibn. ad Bern. Tom. ii.
 p. 347. What a judge of demonstration was this
 gentleman !

THE said Mr. Jones also gave to Dr. Pellet a copy
 of the small tract of fluxions, written by Sir Isaac
 Newton in Nov. 1666. This copy of Dr. Pellet a
 friend lent me, and I found it agreed exactly with
 Sir Isaac Newton’s description.

As it is in English, had it been published †, some
 mistakes, that have been made about the nature of
 fluxions, perhaps occasioned by Sir Isaac Newton’s
 brevity ‡, might have been avoided.

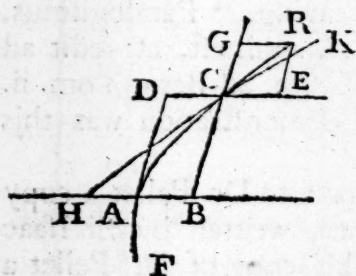
HERE the author often calls the fluxions of lines,
 as well as of other quantities ||, the *velocities* of in-
 crease; the *velocities* with which they increase, &c. as
 will appear from the following quotations.

* Philos. Trans. N° 342. p. 208. Or Comm. Epist. p. 41.
 See also Journal Littéraire 1714. p. 348, &c. † Why he
 did not publish this tract may be learnt from the *Commercium*
Epistolicum, p. 164; he not thinking himself then at an age
 proper for writing. ‡ Discourse, §. 4. || Dissertation, §. 6.

In the scholium to his problem for finding fluxions he says, "Note, that if there happens to be in any equation either a fraction, or surd quantity, or a mechanical one (*i. e.* which cannot be geometrically computed, but is expressed by the area or length or gravity or content of some curve or solid &c.) to find in what proportion the unknown quantity *increases* or *decreases* do thus." What follows is suitable to what is delivered in the Treatise written in 1671. Prob. 1. §. 5—12.

In the Problem for drawing tangents it is said, "With what *velocity*, they" [the flowing lines] "increase or decrease—."

AGAIN, "If the *celerity* of the *increasing* or fluxion of AB be called *p*, and of BC be *q*, I make CE :



GC :: $p : q$:: (CE : ER ::) HB : CB; and the point C shall move in the diagonal line CR (by Lem. 7.) which is therefore the required tangent.

"Now the relation of

p and q may be found

"by the foregoing equation * (p signifying the increase or fluxion of x and q of y) to be $4px^3 -$

$9pyx^2 + 2payx - 2py^3 - 3qx^3 + qax^2 - 6qy^2x$

$- 4qy^3 = 0$. And therefore $HB = \frac{py}{q} =$

$\frac{3yx^3 - ayx^2 + 6y^3x + 4y^4}{4x^3 - 9yx^2 + 2ayx - 2y^3}$; which determines the tangent.

"HENCE may be observed this general Theorem for drawing tangents to curve lines thus referred to straight ones. Multiply the Terms of the Equation, &c." See the abovementioned Treatise, Prob. iv. §. 4.

* viz. $x^4 - 3yx^3 + ayx^2 - 2y^3x + a^4 - y^4 = 0$.

"AGAIN,

AGAIN, in his example of curves described by the intersections of right lines revolving about centers, he says, "That is the celerity of the increase of x being called p and of y being q , $DE : EF :: p : q$. Then shall the diagonal CE be the required tangent." See the abovementioned Treatise, Prob. iv. §. 39.

IN the Problem for determining the curvature of curves are these expressions, "The velocity of the increase, or fluxion of v ," and "The motion of the point C from B (that is the velocity with which $y = CB$ increases) will be q ."

IN the Problem about Quadratures, he says, "The velocities with which they" [areas] "increase, will be as BE to BC " [ordinates]. Again, "The velocities with which the areas ABC and DEF increase."

Now some of these expressions are rendered in Latin thus. In the Principia, "*Velocitates incrementorum ac decrementorum* *;" in Dr. Wallis's Works †, "Per earum fluxionem intelligit" [Newtonus] "*celeritatem incrementi vel decrementi*;" and in the Quadratures, "*Incrementorum velocitates*," and "*celeritates crescendi*."

BUT the expression *Velocitates incrementorum ac decrementorum* being usually translated, The velocities of the increments and decrements ‡, has occasioned Sir Isaac Newton to be misunderstood by the unskilful ||; they not attending to his expression *Celeritates crescendi*, which might have pointed out to them the real sense of the other expressions ||†.

* Lib. 2. Lem. 2. p. 244. and Dissertation, §. 77.

† Vol. II. p. 391. ‡ See Mr. Motte's Translation of the Principia, as also the Translation of the Quadratures in Dr. Harris's Lexicon Technicum, Vol. II. || Discourse, §. 87. Account, §. 4, 5. Review, §. 1. Dissertation, §. 6, 97.

||† Thus his expression *quantitates nascentes* having been always translated *nascent quantities*, has led many into mistakes. See Discourse, §. 107. Dissertation, §. 72.

IN this Tract Sir Isaac Newton determines the relations of fluxions both in the manner he had done in his paper of the 13th of Novemb. 1665, and also by multiplying the terms of the equation by arithmetical progressions. In the demonstration he used indivisibles *; saying in the premised Lemma, "And though they" [the bodies or points describing the flowing lines] "move not uniformly, yet are the *infinitely little* lines, which each moment they describe, as the velocities, which they have, while they describe them." But in the following Problems he makes no use of indivisibles at all, nor of his method of prime and ultimate ratios †, ever expressing fluxions by finite right lines, that are to one another in the same proportion with them ‡.

As in drawing tangents, he supposes no part of the tangent to coincide with the curve, but to lie in the diagonal of a parallelogram, whose finite sides are to each other as the fluxions of the varying lines, by whose intersections the curve is described. Hence in his description of fluxions, in the Introduction to the Quadratures, he says of them; "exponi autem possunt per lineas quascunque quæ sunt ipsis proportionales." And he seems to have had this old tract in his eye, when he said

* Account, §. 45. Dissertation, §. 93. † Ibid. §. 10, 93.

‡ In his papers concerning the center of gravity, he indeed uses indivisibles: as in the second Problem, which is this, "To find such plane figures, which are equiponderate to any given plane figure in respect to an axis of gravity in any given position," he thus expresses himself, "The motions whereby x and y do increase or decrease, I do call p and q . Now the ordinates multiplied into their motions p and q may signify the *infinitely little* parts of those areas, which each moment they describe, which *infinitely little* parts do equiponderate (per Lem. 1 & 2.) if they multiply'd by their distances from the axis AK do make equal products;—And if all the respective *infinitely little* parts do equiponderate, the superficies must do so too."

(speak-

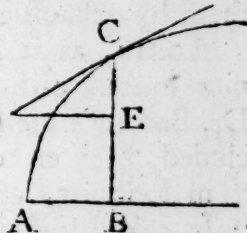
(Speaking of the comparison of his method with that of M. Leibnitz) "It is more elegant, because in his calculus there is but one *infinitely little* quantity represented by a symbol, the symbol *o*. We have no ideas of infinitely little quantities, and therefore Mr. Newton introduced fluxions into his method, that it might proceed by finite quantities as much as possible *."

NEXT he determines "The quantity of the curvature of lines" by finding "that point fixed in the curve line's perpendicular, which is then in least motion; "for" [says he] "it is the center of a circle which passing through the given point is of equal curvature with the line at that given point." Here the velocities of the points in the radius of the curvature intersecting the curve and the abscisse are most ingeniously expressed by finite right lines, that are used in the solution of the problem.

THENCE he determines the points between the concave and convex portions of curves, as also where they are most or least curved.

THE methods of squaring and rectifying curves are there given after the same manner, as in his book written in 1671. But there is one problem relating to the latter subject thus delivered.

To find the nature of a curve whose length is expressed by any given equation (when it can be.)



R E S O L U T I O N.

LET AB, BC, AC be expressed by x , y , z , and their motions by p , q , r , respectively, and let the relation between x and z be given. Then finding the relation between p and r , make $\sqrt{rr - pp} = q$;

* Phil. Transf. N^o 342. p. 205. Or Com. Epist. p. 38.
Z 2 for

for drawing CD a tangent to the curve at C, and DE perpendicular to BC, the lines DE, EC, DC, shall be as p, q, r ; but $\sqrt{DC^2 - DE^2} = EC$, therefore $\sqrt{rr - pp} = q$. Now the ratio between x and $\frac{p}{q}$ being thus found, find y , and the relation between x and y determines the nature of the curve.

IN this discourse Sir Isaac Newton finds infinite series's by division and extraction of roots. But as he here for extracting the roots of adaffected equations refers to Vieta's Exegesis; he seems not to have then discovered the Compendium, which he found out a very little time after, and fully described in the Tract, that was sent to Mr. Collins in 1669*.

It has been lately said † of Sir Isaac Newton, — “il a trouvé le calcul différentiel, en ne faisant que généraliser la méthode de Barrow pour les tangentes.” Whereas Sir Isaac Newton found out his method by considering the proportions of the velocities of increasing, or fluxions of quantities generated together by motion. These proportions he demonstrated in the beginning by indivisibles, and afterwards by his method of prime and ultimate ratios. It was Mr. Robins, who first demonstrated them by exhaustions.

AGAIN, as it has been supposed, that M. Leibnitz indeed deduced the differential calculus from Dr. Barrow's method of tangents; it has been concluded ‡ “en ce cas ce ne seroit, ni Newton, ni Leibnitz, ce seroit Barrow qui auroit trouvé le calcul différentiel.” But is this logic? For though we believe, M. Leibnitz by the hints given him in Sir Isaac Newton's letters, was enabled to deduce his method from that of Dr. Barrow; yet we know, that Sir Isaac Newton had discovered

* Comm. Epist. p. 75.

† Encyclopédie, Tom. vii.

p. 119.

‡ Ibid. Tom. iv. p. 988.

his method about five years before Dr. Barrow's Geometrical Lectures were published *.

AND Dr. Barrow approved of these inventions, and was so well satisfied with young Newton's superior genius, that he gladly submitted his own works to his examination and correction †; and resigning to him the Professorship of the Mathematicks, he wholly dedicated his future studies to Divinity; which produced those most excellent discourses, that have acquired him a universal and lasting fame.

SIR Isaac Newton has often by foreigners been represented as a scholar of Dr. Barrow; but this is a mistake, he having had no instructor (as indeed in these studies is often the case) and falling on mathematical treatises by chance, such as were very improper for a beginner; yet without any one's assistance, by his own unequalled sagacity and invention, he not only overcame the difficulties of the subjects, increased by the manner, they were delivered in the books he read; but immediately made improvements, and very soon laid the foundation for all the marvellous discoveries, he afterwards produced. As he was very rapid in the course of his inventions; so he seems to have been from time to time tired in their pursuit ‡; and to have resumed those studies at two or three reprises only. Insomuch that a small portion of his long life was employed in these speculations.

BUT to return to our narration; Mr. Jones also gave the said Dr. Pellet a copy of Sir Isaac Newton's Treatise of series and fluxions, written in 1671 §.

* If we may presume to guess, perhaps Sir Isaac Newton took the hint of his method of fluxions from what Mersennus said of M. de Roberval's inventions. See his Ballistic. p. 115.

† Barrow, Lect. Optic. in the preface. ‡ Comm. Epist. p. 141. § He also let Dr. Taylor see this Treatise, whence it is referred to in his Methodus Incrementorum, p. 64.

This was deficient in several places; for Mr. Jones was wont to curtail or otherwise disguise the papers, he communicated to his scholars, that none might make out a compleat book.

THE translation Mr. Colson has published of this treatise was from Mr. Jones's own copy; which, I believe, was very perfect, as far as Sir Isaac Newton had at first composed it; as well as I can remember from my having read many years ago the original manuscript, when it was in my friend Dr. Pemberton's custody.

IN this treatise Sir Isaac Newton chose the method of determining the relation of fluxions, by multiplying the terms of the equation by arithmetical progressions, which he had before described in his abovementioned Tract written in November 1666. "This indeed," says the Translator in his Comment, p. 242, "is not so short as the method of taking fluxions, which he elsewhere delivers*, and which is commonly followed; but it makes sufficient amends by the universality of it, and by the great variety of solutions it will afford. For we may derive as many different fluxional equations from the same given equation, as we shall think fit to assume different arithmetical progressions."

M. Van Hudden in his Tract, printed at the end of the first volume of Cartes's Geometry in 1659, on the maxima and minima, after having given a rule † for determining these from an equation containing one variable quantity only, which is altogether similar to Sir Isaac Newton's first method of taking fluxions; subjoins the same remark Mr. Colson has here made, that by the use of such arithmetical progressions [as Sir Isaac Newton proposes] the method would be rendered more general and useful ‡. But

* In Dr. Wallis's Op. Math. Vol. II. p. 392; and in the Quadratures.

† Pag. 511.

‡ Ibid. p. 513.

this implex method Sir Isaac Newton afterwards laid aside, observing the other to be preferable*, and for this he had very good reason: for in reality nothing more is effected by these different progressions, than combining with the simple form of the fluxionary equation some multiple of the original equation divided by the flowing quantity.

HERE, as in the *Analysis per æquationes numero terminorum infinitas*, moments, as infinitely small parts, are introduced; accordingly in Dr. Wallis's works it is said †, "*quamvis fluentes quantitates et earum fluxiones prima fronte conceptu difficiles videantur, earum tamen notionem cito faciliorem evasuram putat [Newtonus] quam sit notio momentorum aut partium minimarum vel differentiarum infinite parvarum.*—Attamen non negligit theoriam talium partium—." Hence Sir Isaac Newton, in the Introduction to his *Quadratures*, says, "Peragi tamen potest analysis in figuris quibuscunque seu finitis seu infinite parvis quæ figuris evanescentibus finguntur similes, ut in figuris quæ per methodos indivisibilium pro infinite parvis haberi solent, modo caute procedas."

THIS tract Sir Isaac Newton intended to finish and publish with his *Lectiones Opticæ* ‡, that he was then reading in the university of Cambridge, and wherein he had mentioned it ||. But disputes arising from what was already extant of his on the subject of light and colours in the *Philosophical Transactions*, he resolved not to print those Lec-

* In his remarks on the *Judicium primarii Mathematici*, he observes the method delivered in his *Quadratures* is the most perfect of any in these words; "*Methodus fluxiones omnes capiendi, seu [in the style of M. Leibnitz] differentiandi differentialia habetur in Propositione prima Libri de Quadraturis: et est verissima et optima.*" *Comm. Epist. p. 249.*

† Vol. II. p. 391. ‡ Second letter to Mr. Oldenburgh in *Comm. Epistolic. p. 165.* || Part. i. Sect. iv. Prop. 33, 34.

tures*. However, his Treatise of series and fluxions was to accompany a translation out of Dutch of Kinckheysen's Algebra †, that Mr. Pit, an eminent bookseller of London, had procured; and which Sir Isaac Newton was desired to improve and publish. But he at length, not liking this proposal, inserted many of his improvements in the public Lectures, intended to contain a general system of algebra, which he read about the year 1675.

In that Treatise, he mentions prime and ultimate ratios ‡; but he never finished it, being then employed in discovering some of the propositions, that make the subjects of his Principia, and in writing his Treatise of Quadratures ||; a case of the 5th proposition of which, together with the forms of curves, that might be compared with the conic-sections, as also cyphers, containing the two first problems of his Treatise of series and fluxions, written in 1671, with a description of his method of extracting the fluents out of equations involving fluxions, and also of assuming an arbitrary series, were sent to M. Leibnitz, in a letter dated Octob. 24, 1676 †||. Notwithstanding all which M. Leibnitz has, even long after the cyphers had been explained, asserted, he believed Sir Isaac Newton at that time had no knowledge of the characteristic and algorithm of fluxions ‡||, that is, he was ignorant of what he had written in a paper so long ago as the 13th of November, 1665.

WHEREAS Sir Isaac Newton has truly said, that he had then made his method of fluxions much more universal, than the differential method of M. Leibnitz is at present. "For when the method of

* Comm. Epist. p. 165. † Ibid. p. 101. ‡ Pag. 236, 237, of the second edition at London, in 1722. || Phil. Trans. N^o 342. p. 202, 206. Or Com. Epist. p. 34, 40. †|| Com. Epist. p. 166, 167, 173, 188. ‡|| Raphs. Hist. Fluxion. p. 97. or Recueil de Divers Pieces, &c. Tom. ii. p. 4.
" fluxions

“ fluxions proceeds not in finite equations, he re-
 “ duces equations into converging series by the
 “ binomial theorem, and by the extraction of
 “ fluents out of equations involving or not in-
 “ volving their fluxions. And when finite equa-
 “ tions are wanting, he deduces converging series
 “ from the conditions of the problem, by as-
 “ suming the terms of the series gradually, and
 “ determining them by those conditions. And
 “ when fluents are to be derived from fluxions,
 “ and the law of the fluxions is wanting, he finds
 “ the law *quam proxima* by drawing a parabolick
 “ line through any number of given points*.”

All this, together with the comparing curves with others, that are more simple, is shewn in the small treatise entitled *Analysis per series, fluxiones et differentias quantitatum*, published by Mr. Jones at London in 1711.

THUS Sir Isaac Newton had been successively, by sundry accidents and his own unwillingness to appear in publick †, prevented hitherto from publishing his treatises on fluxions. This delay was not the effect of any formed resolution to conceal his inventions, that he might be the more admired in what he should be enabled to perform by their assistance in any future work: it does not appear, that he had any express intention of writing his *Principia*, till Dr. Halley accidentally suggested to him that design.

ON the coming out of that immortal book, these discoveries of Sir Isaac Newton became to be much talked of; so that Dr. Wallis pressed their author to publish something further of his method of

* *Phil. Trans.* N° 342. p. 193. Or *Comm. Epist.* p. 25.

† “ Je ne parle point ici de Part avec lequel il [Newton] avoit caché sa méthode des fluxions, la chef de toutes ses sçavantes recherches—.” Says M. Clairaut in the *Mem. of the Royal Academy of Sciences* for 1745. p. 329.

fluxions; which occasioned Sir Isaac Newton to send to him, with other things relating to the subject, the first proposition of his Treatise on the quadratures of curves, wherein is determined the several orders of fluxions. These particulars the Doctor printed in the second volume of his mathematical works in 1693*; three years before M. Leibnitz had given his rules for finding those orders in differentials.

NOTWITHSTANDING this, it was said in the preface to the *Analyse des infiniment petits* in 1696 by M. de Fontenelle (who, we are more than once told †, wrote that preface), “ Mais le caractère stique de M. *Leibnitz* rend le sien beaucoup plus facile et plus expeditif; outre qu’elle est d’un secours merveilleux en bien des rencontres ‡.” Does not this answer to what M. d’Alembert has said on another occasion, “ Notre fureur d’écrire avant que de penser, et de juger avant que de connoître ||.” For M. de Fontenelle had no reason to give the preference to M. Leibnitz’s method on any account whatever, unless he had persuaded himself, that a *d* was more easily writ than a tittle.

BUT this partiality is still kept up, and one of the authors of the *Journal des sçavans* †||, in giving an account of Mr. Maclaurin’s book, has thought fit to say, “ M. Leibnitz a représenté les *différences* par la caractéristique *d*, ce qui est infiniment commode.” This fancy can only arise from having been accustomed to that letter; and so, on the contrary, others think Sir Isaac Newton’s characteristics for distinguishing the different orders of fluxions

* Pag. 392. † *Encyclopédie*, Tom. i. at Paris in 1751. p. 677. and Tom. vii. Ibid. An. 1757. p. 630. ‡ See *Philos. Transact.* N° 342. p. 204. Or *Com. Epist.* p. 37.

|| *Encyclop.* Tom. v. in the Advertisement, p. 4. at Paris in 1755. †|| For the month of August 1750.

by as many points to be much more commodious *.

THERE has indeed of late been alledged important reasons for the preference of that of M. Leibnitz. To make this the more apparent, there has been presented to our view a symbol with a dozen points over it, to denote a fluxion of that order; as if any such order would ever be used, or that 12 points would not be as easily told, as 12 *ds*.

If it is not ridiculous to be serious upon so trifling an object, may it not be reasonably urged against the use of this letter *d*, that it took its rise from the term and idea of differentials, which are either erroneous quantities, or have no being in nature; so that *dx*, *dy*, &c. were introduced to denote false quantities not bearing the proportions to one another, that were required; or imaginary quantities incapable of any proportion.

BUT it is asserted, “ Introduire ici le mouvement, c’est y introduire une idée étrangere, et qui n’est point nécessaire à la demonstration : d’ailleurs on n’a pas d’idée bien nette de ce que c’est que la vitesse d’un corps à chaque instant, lorsque cette vitesse est variable. La vitesse n’est rien de réel ; c’est le rapport de l’espace au temps, lorsque la vitesse est uniforme. Mais lorsque le mouvement est variable, ce n’est plus le rapport de l’espace au temps, c’est le rapport de la différentielle de l’espace à celle du temps ; rapport dont on ne peut donner d’idée nette, que par celle des *limites*. Ainsi il faut nécessairement en revenir à cette dernière idée, pour donner une idée nette des fluxions.” *Encyclopédie*, Tom. vi. p. 923.

THAT motion is not foreign to geometry, we know from the example of the ancients, who ad-

* *Philos. Trans.* N° 342. Or *Comm. Epist.* p. 30. And *Journ. Liter.* for July and August, p. 342.

mitted it in the descriptions of figures *; and Proclus has said, " that this science takes cognisance of " magnitudes and figures, and of the limits attending them, and of the ratios they bear to each other, and of the affections, various positions, " and motions, to which they are subject †."

THE introducing motion into geometry may be of great use. Some have thereby endeavoured to overcome the difficulty about parallel lines, which has been so much canvassed amongst the mathematicians. And after the great Galileo had shewn, how bodies descended with an accelerated velocity, whereby being projected obliquely, they described in their motions parabolas; the geometers, as Torricellius, Roberval and others began to consider the effects of such motions in the descriptions of curvilinear figures, and applied that idea to the determining their tangents, &c. See Torricellius de Motu gravium, p. 121. Lettera di Timauro Antiati [Sign. Dati] p. 14. Mersen. Ballistic. p. 115. Dettonville [M. Pascal] de la Roulette, p. 6.

THIS gentleman in his second objection contradicts himself, first in asserting that there is nothing real in velocity, which is as much as to say, that it has no existence in nature, and at the same time pretends to define it, though indeed in terms, that have no real meaning; for he tells us, that velocity is the proportion of space to time, thus confounding the two separate ideas of velocity and proportion, and without specifying what space and what time he means; nor is it very necessary to inquire, for however explained, the definition is absurd; since space and time being heterogeneous quantities, are incapable of bearing proportion one to the other.

* See Sextus Empericus, Ed. Genev. p. 96, 97. Also Barrow, Lect. Mathem. An. 1664. Lect. iv. p. 69. Newton's Quadratures, in the Introduction; and Philos. Transf. N^o 342. p. 205. Or Com. Epist. p. 38. † Proclus in Euclid. lib. 11. p. 16. at Basil in 1533.

THE idea of motion, and its affection velocity, are very distinct from the ideas either of time or space; for both of these may be present to the mind without the other. True it is, that different degrees of velocity in the motion of any body are in the same proportion to each other, as the spaces, which would be described by those respective velocities, were they uniformly continued for any one specified portion of time, whether those velocities remained long enough uniform for the moving body actually to describe those spaces or not. And how such finite spaces are to be applied for measuring the degrees of velocity at each point of time in bodies, whose motion is continually varying, is distinctly explained by Mr. Robins in his Treatise of fluxions, without having recourse to any imaginary differential of time, as this author expresses himself, of which he truly says, we have no clear idea; he should have said no idea at all.

BUT at length Sir Isaac Newton himself gave an accurate account of his method of fluxions in the Introduction to his Treatise of Quadratures, printed at the end of his Opticks in 1704. This Treatise of Quadratures he was prevailed on to publish; because, as he had lent it out *, he observed some things had been copied from it †. The Introduction exhibited an idea of his method of fluxions entirely free from the notion of infinitesimals ‡.

M. Leibnitz also had took upon him to correct the notion of indivisibles, but unhappily. For as he had called his differential method Analysis indivisi-

* Advertisement before the Opticks; Comm. Epist. p. 222, in the note †; and Raphson. Hist. Fluxion. p. 2. † Joan. Craig. de Calculo Fluentium, printed at London in 1718, in the preface.

‡ “ In finitis quantitibus analysin sic instituere, “ et finitarum nascentium vel evanescentium rationes primas vel “ ultimas investigare, consonum est geometriæ veterum, et volui “ ostendere quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere.” Introd. ad Quadratum, bilium,

bilium, so now he began to name it Methodus in-
comparabilium. He says, "siquis nolit adhibere
" *infinite parvas*, potest assumere tam parvas quam
" sufficere judicat, ut sint incomparabiles, et errorem
" nullus momenti, imo dato minorem producant."
Again "et possunt adhiberi triangula communia in-
" *assignabilibus* illis similia" &c. And afterwards he
adds, "potest etiam exprimi conatus centrifugus
" per differentiam radii et secantis ejusdem anguli
" cujus differentiae discrimen a sinu verso est infinite-
" sies, infinities, infinite parvum, adeoque nullissimum
" respectu radii *." Against this arrant nonsense,
which however was esteemed as containing pro-
found mysteries †, Sir Isaac Newton seems to have
levelled that passage of the Introduction to his
Quadratures, "errores quam minimi in rebus mathe-
" maticis non sunt contemnendi;" and in his demon-
stration of his rule for finding fluxions, he has care-
fully expunged, whatever might favour indivisibles.

DID this description of Sir Isaac Newton, in op-
position to M. Leibnitz's crude conception, move
the spleen of the authors of the *Acta Eruditorum* ‡,
when in giving an account of the Quadratures, they
represented, that, what is taught there, was pub-
lished by Mr. Craig and Dr. Cheyne ||? But their
writings contain only an inconsiderable part of what
was sent to M. Leibnitz in 1676. And Mr. Craig
has owned, that he was obliged to Sir Isaac Newton
for correcting a deficiency in his method ||†.

* *Act. Erudit. Mens. Feb. 1689.* † M. d'Alembert has
well observed, "en general les hommes ne haïssent point l'ob-
" scurité, pourvu qu'il en résulte quelque chose de merveilleux."
Encyclop. Tom. iv. p. 988. Perhaps if it had not been for this
marvellous obscurity, the differential method would never have
acquired the pompous name of the sublime geometry.

‡ *Act. Erudit. Mens. Januar. 1705.* || M. Leibnitz in a
letter to M. Bernoulli shews his judgment in speaking slightly
of Sir Isaac Newton's Quadratures. *Epist. Leibn. ad Bern.*
Tom. ii. p. 124. ||† *Philos. Trans. for 1686. N° 183.*

AGAIN, as in that account insinuations were thrown out against Sir Isaac Newton's candour*; it occasioned the controversy about the invention of fluxions.

THIS account was from its style, and other circumstances, supposed to have been written by M. Leibnitz himself†. He therefore, in a letter to M. Bernoulli‡, pretended to be ignorant of it, and gives a very odd reason, why he ought not to be thought its author. There, says he, I hear is praised M. Tchernhaus's discoveries, which I do not esteem. But M. Leibnitz has not disdained such artifices; for in publishing the *Judicium primarii Mathematici*, in order to conceal the writer, he rendered his friend M. Bernoulli ridiculous, by making him quote himself under the title of *eminens quidam mathematicus*§. However, M. Leibnitz afterwards justified the account, and endeavoured though in vain to explain away its most exceptionable sense.

CONCERNING the invention of fluxions, there have been published many tracts†§, whence may be learnt the merits of the cause; but more especially from the second edition of the *Commercium Epistolicum*, printed at London 1722 in 8°; where Sir Isaac

* This rancour M. Leibnitz had long suppressed. In a letter to M. Bernoulli, dated 23 Aug. 1696, he says, "certum est me domino Newtono, ante viginti annos meæ methodi differentialis fundamenta communicasse, antequam ille mihi quicquam de suis huc spectantibus. An nonnihil inde profecerit haud satis scio: neque ideo dicere aufferim." *Leibn. et Bern. Epist.* Tom. i. p. 195.

† *Ibid.* Tom. ii. p. 308. ‡ *Ibid.* p. 313. § *Ibid.* p. 330. Here M. Leibnitz did not prudently follow his friend's advice, who had signified before to him, "rogo vero, ut quæ hic scribo, iis recte utaris, neque me committas cum Newtono ejusque popularibus; nollem enim immisceri hisce litibus." *Ibid.* p. 311. And indeed it proved of ill consequence; for it engaged him in controversy with Dr. Keill, who exceeded M. Bernoulli in common sense, though he fell far short of him in mathematical invention.

†§ *Journal Littéraire* for July and August 1714, and for 1716. Tom. viii. Part 2. *Raphs. Hist. Fluxion.* at the end; and *Recueil des Divers Pieces sur la Philosophie*, &c. Tom. i. in the preface, pp. 11—66, and Tom. ii. pp. 3—124.

Newton

Newton has, in the Preface, Account and Annotation, which were added to that edition, particularly answered all the objections M. Leibnitz and M. Bernoulli were able to make, since the *Commercium* first appeared in 1712.

THIS *Commercium* is composed of ancient letters, and parts of letters digested in the order of the time they were written, and relates to the invention of the methods of infinite series, moments and fluxions. At the bottom of the pages are added several judicious notes, that tend greatly to illustrate the whole affair.

To this *Commercium* M. Leibnitz long threatened to oppose one of his own *; and upon M. Bernoulli's hearing there was a large account of it published in the *Philosophical Transactions* †, he urged M. Leibnitz to hasten the fulfilling his promise ‡.

* *Mihi consilium est, edere aliquod Commercium Literarium meum, unde apparebit quam in aliis quoque Newtonus olim tenuis fuerit.* Leibn. et Bern. Epist. 30 Decemb. 1714. Tom. ii. p. 341.

† N° 342. This account is also published in Latin before the second edition of the *Commercium Epistolicum*. It was wrote by Sir Isaac Newton himself, and the arguments there used are unanswerable, which M. Bernoulli, in complaisance to M. Leibnitz, without having seen them, is pleased, in a letter to that gentleman, to call after him, *argutationes*, Tom. ii. p. 364, 367. He in another place, Ibid. p. 377, supposed Sir Isaac Newton had acted under a mask; and the authors of the *Acta Erudit.* [An. 1720. Mens. Mar. p. 137] say, Sir Isaac Newton's arguments were the same with Dr. Keill's. As to that, it cannot be doubted but the Doctor was assisted by Sir Isaac's conversation, and by the excellent notes in the *Commercium Epistolicum*, But the Doctor, in his writings, making use of some asperities, suitable to the affronts he had received, not being agreeable to Sir Isaac Newton's temper, he thought fit to publish a dispassionate account of the affair himself. At last Sig. Conti prevailed on Sir Isaac Newton to appear himself in this controversy. What he wrote was published in English at the end of *Raphs. Hist. Fluxion.* and in French in the *Recueil &c.* The substance of which Sir Isaac Newton afterwards included in the preface to a second edition, he made, of the *Commercium Epistolicum*.

‡ “*Neceffe puto, ut nonnihil matures alterum illud, quod paras, Commercium Epistolicum priori ab Anglis edito opponendum.*” Leibn. et Bern. Epist. 19 Jan. 1716. Tom. ii. p. 367.

M. Leib-

M. Leibnitz loudly complained of the partiality of the collection, and represented the notes as malignant and false *. He attempted to give instances of the first, but quite missed his aim †; and against the notes he never produced any thing besides assertions; so that Sir Isaac Newton himself has well observed in their justification, “interpretationes illæ nullius quidem sunt auctoritatis, nisi quam ab epistolis derivant, sed male fundatas esse Leibnitius nunquam ostendit ‡.”

AGAINST the *Commercium*, M. Leibnitz and M. Bernoulli used to urge, that it regarded the business of infinite series alone; whereas there is sufficiently considered not only that speculation, but also the affair of moments and fluxions ||. It is no wonder, these gentlemen disliked the treating here so largely on infinite series; when thence was manifest M. Leibnitz's unfair practices.

AGAIN they pretended, that it was not likely Sir Isaac Newton found out the method of fluxions; since in his *Principia*, published in 1687, he had mistook in regard to their higher orders, and that he gave, in the *Scholium* at the end of his *Quadratures* published in 1704, a faulty rule for determining those orders †||. But it has been often demonstrated, that in the *Principia* he committed no error on that account ‡||; and in 1693 was published by Dr. Wallis ||† the first proposition of Sir Isaac Newton's *Quadratures*, containing a right rule for finding all the orders of fluxions; and in 1711 Dr. Keill shewed by a series how to make out those

* Raphs. Hist. Fluxion. pp. 97, 104; or Recueil &c. Tom. ii. pp. 5, 36, 47, 52, 53.

† Ibid. pp. 98, 101, 105, 113; or Ibid. pp. 5, 18, 53, 82; and Comm. Epist. at the beginning of the preface.

‡ Com. Epist. in the preface, p. 3. || Journ. Liter. for July and August, p. 322. Raphs. Hist. Fluxion. p. 112; or Recueil &c. Tom. ii. p. 80.

†|| Com. Epist. p. 246. || See above, in p. 335, the note markt ‡||.

||† Op. Math. Vol. II. p. 392.

orders *, which he learnt from Sir Isaac Newton †. As to the mistake in the Scholium to the Quadratures, it may be observed, that Sir Isaac Newton gave to M. Bernoulli's nephew, when in London, a corrected copy ‡; but as this mistake was afterwards much urged against Sir Isaac Newton, both Dr. Keill and he made it plainly appear to have been a mere oversight ||.

THEY further said, it was not probable, that M. Leibnitz did not find out the infinite series for measuring the circle; when both Sir Isaac Newton and Dr. Gregory allowed him that invention.

Now in 1669, amongst other series's of Sir Isaac Newton, one for finding the arch of a circle from the sine †||, and in 1671 another of Mr. Gregory for finding that arch from the tangent ‡||, were sent to Mr. Collins; who was very free in communicating these discoveries. In 1674 M. Leibnitz mentions, in a letter to Mr. Oldenburgh, his being possessed of the first series §||; and the next year Mr. Oldenburgh sends both Sir Isaac Newton's and Mr. Gregory's series's to M. Leibnitz ||*. But in 1676 M. Leibnitz, dropping his pretensions to the first series, not being able to demonstrate it, sends to Mr. Oldenburgh, as his own, that of Mr. Gregory with a demonstration **. Sir Isaac Newton and Dr. Gregory scrupled not to allow M. Leibnitz found out this series; for they knew nothing of Mr. Oldenburgh's letter, which lay buried for above thirty years amongst the papers of the Royal Society †*. This discovery startled at first M. Bernoulli ‡*;

* Com. Epist. p. 232. † J. Keill. Epist. ad J. Bern. p. 19.

‡ Epist. Bern. ad Lejbn. Tom. ii. p. 310. || Journ. Liter. July and August 1714, p. 348. Epist. J. Keill ad Bern. p. 15; and Philos. Trans. N° 342. p. 208; or Com. Epist. p. 41.

†|| Comm. Epist. Collin. p. 85. ‡|| Ibid. p. 98.

§|| Ibid. p. 116. ||* Ibid. p. 118, &c. ** Ibid. p. 148.

†* Ibid. p. 118. ‡* Lejbn. et Bern. Comm. Epist. Tom. ii. p. 309, 317.

but

but he was too deeply engaged in M. Leibnitz's defence to retract, though M. Leibnitz once expressed his apprehensions of it *. In short, M. Leibnitz was at last, in 1713, forced to acknowledge Mr Gregory to be the original author †; but why he did not acknowledge as much at first, cannot be fairly accounted for.

OF the same force were produced, by these acute reasoners, in opposition to the *Commercium Epistolicum*, other arguments, consisting only of surmises and presumptions against evident matters of fact.

BUT from that *Commercium* it is most manifest, that Sir Isaac Newton had discovered his method of fluxions many years before M. Leibnitz's pretensions. Hence M. de Montmort has candidly said, " Pour moy je soutient icy et je l'ai toujours soutenu hautement que M. Newton a été maître du calcul différentiel et integral avant tout autre géometre, et que de l'année 1677 il sçavoit tout ce que les travaux de M. Leibnitz et M. Bernoulli ont découvert depuis ‡."

BESIDES, M. Leibnitz, from the whole tenour of his conduct, may be justly suspected of having learned by information, what he pretended to be the inventor of. For he pretended to Mouton's differential method ||; to a property of a series, that had been published by M. Pascal †||; to Sir Isaac Newton's series for measuring a circular arch from the sine ‡||; to a series of James Gregory for the same thing from the tangent §||; to four other series's of Sir Isaac New-

* Leibn. et Bern. Comm. Epist. Tom. ii. p. 313.

† Ibid. p. 341. ‡ Letter to Dr. Taylor, dated Jan. 22, 1717. at the end of Keill's letter to Bernoulli.

|| Philos. Transf. N^o 342 p. 183; or Comm. Epist. p. 13.

†|| Ibid. p. 215; or Ibid. p. 49. ‡|| Ibid. p. 184; or Ibid. p. 14.

§|| Journal Liter. for July and August 1714, p. 353, &c. and Philos. Transf. N^o 342. p. 185; or Com. Epist. p. 15; and Keill Epist. ad Bernoulli, p. 19.

ton*; to a method of regression†; to the Differential Analysis at a time, when it was certain, he was ignorant of it‡; and, lastly, to some of the principal propositions of the Principia||.

By these instances it abundantly appears, how freely M. Leibnitz could appropriate the inventions of others; and it is farther shewn, how easily, from the manner he first produced this discovery in his letter to Mr. Oldenburgh, dated the 21st of June 1677†||, it might be deduced from Dr. Barrow's Method of tangents, by the assistance of what is declared in Sir Isaac Newton's letters‡||, which had been sent to M. Leibnitz, especially in that of the 10th of December 1672. And hence it is, that the committee of the Royal Society in their report refer particularly to that letter‡*.

No doubt others, by the help of these hints, might have discovered this method; as Mr. Gregory did so from much less information; though his candour would not permit him to prevent the first inventor||*.

BUT M. Leibnitz's different manner of producing this method, first in his letter to Mr. Oldenburgh, and afterwards in the Acta Eruditorum**, is very observable. In the former, it appears readily deducible from Dr. Barrow's Differential method of tangents; in the other, the resemblance is disguised as much as possible, and it is delivered with an affected and astonishing obscurity.

BESIDES, there is good grounds to suspect M. Leibnitz had a sight of Sir Isaac Newton's Analysis

* Phil. Trans. N^o 342. p. 188; or Com. Epist. p. 19.

† Ibid. p. 189; or Ibid. p. 20. ‡ Ibid. p. 192; or Ibid. p. 24. || Journal Liter. for July and August 1714, p. 348; and Philof. Trans. N^o 342. p. 208; or Com. Epist. p. 42.

†|| Comm. Epist. p. 192. ‡|| Journal Liter. 1714, p. 325; and Philof. Trans. N^o 342. p. 191, 192, 196, 197; or Com. Epist. p. 23, 24, 27, 28, 31. ‡* Com. Epist. p. 242, 243. ||* Com. Epist. p. 104. ** Ann. 1684. Mens. Octob.

per æquationes numero terminorum infinitas *; for he early put in a claim to a series contained in that Tract†, and afterwards has owned, he had seen before, in the hands of Mr. Collins, letters of Sir Isaac Newton‡; Mr. Collins being very forward to boast of, and shew to mathematicians, Sir Isaac Newton's discoveries ||.

HOWEVER, it has been said†||, no doubt to make us believe, M. Leibnitz was capable of inventing the Differential calculus, that he could not learn from Sir Isaac Newton's letters the artifice mentioned in them‡||, of performing the operation without being obliged to reduce the equation.

A REMARK like this has already been taken notice of by Dr. Keill ||*; and I shall farther observe, that the artifice here alluded to is not difficult to find out when once proposed; especially by one, who could be no stranger to M. Fermat's general rule of freeing equations from fractions and surds †*. Yet it is highly probable, M. Leibnitz did not discover this artifice by himself; but might have had it of M. Van Hudden. For M. Leibnitz, in his letter to Mr. Oldenburgh, dated Amsterdam, 28 Novemb. 1676, says, "Amstelodami cum Huddenio locutus sum; cui negotia civilia tempus omne eripiunt.—Præclara admodum in ejus Schedis supereffe certum est. Methodus Tangentium a *Slusio* publicata dum illi fuit nota. Amplior ejus Methodus est,

* How the method of fluxions might be gathered from that treatise, see the notes on it, as it is published in the Commercium Epistolicum.

† Philos. Transf. N° 342. p. 184; or Com. Epist. p. 14.

‡ Raphs. Hist. Fluxion. p. 98, 106; or Recueil &c. Tom. ii. p. 5, 56; and Epist. J. Keill ad J. Bernoul. p. 5.

|| Journal Litteraire for July and August 1714, p. 328.

†|| Encyclopédie, Tom. iv. p. 988.

||* Journal Litteraire for July and August 1714, p. 325.

†* See amongst Cartes's Letters, Vol. III. Let. LXXV, LXXVI; and Fermat's Varia Opera Mathematica, p. 60.

“quam quæ a *Slusio* fuit publicata *.” Now this method of tangents has been since published †, and the editor observes, that from M. Van Hudden’s papers it appears, he had improved his method; so that it proceeded without being obliged to reduce the equation. Besides, M. Leibnitz has owned, M. Huygens was master of the same artifice; whom he has acknowledged to have been his instructor in these studies ‡.

Thus M. Leibnitz, from the notices given him by Sir Isaac Newton, and thereupon consulting the writings of Van Hudden, Barrow, and Slusius ||, at length got at what Sir Isaac Newton had described; and though in his letter, of the 21st of June 1677, he says, what he there explained, he believed to be what Sir Isaac Newton concealed; yet afterwards in publishing it, he did not, as he ought, declare the information, he had received §§; and what is worse, when it had been demonstrated, that Sir Isaac Newton had long before discovered these methods, he and M. Bernoulli by weak arguments endeavoured to shew the contrary.

THESE have often been fully answered by Sir Isaac Newton himself. In his doing of which M. Leibnitz has complained of want of politeness. But is it not a jest for any one to expect insipid flattery, which mostly passes under that denomination, from another, whom he had so grossly and falsely abused **?

* See Wallis’s Op. Math. Tom. iii. p. 646; and Comm. Epist. p. 190, 191.

† In the Journal Litteraire for July and August 1713, p. 455, in a letter of M. Van Hudden to M. Van Schooten, dated Leyden 21 Novemb 1659.

‡ Raph. Hist. Fluxion. p. 98. “ce n’est qu’en France que j’ay pris entré et “M. Huygens m’en donna l’entrée.” This passage is omitted at p. 5. Vol. ii. of the Recueil &c. See also Raph. Hist. Flux. p. 107; or Recueil &c. Vol. ii. p. 58.

|| Comm. Epist. p. 192; also Philos. Transf. N^o 342. p. 192, &c. and p. 216, 219. or Comm. Epist. p. 23, &c. and p. 50, 53.

§§ Phil. Transf. N^o 342 p. 220; or Com. Epist. p. 54.

** In an infamous paper, dated 29 July 1713.

M. Leibnitz

M. Leibnitz insisted to the last, in contradiction to what had been demonstrated, that he was first possessed of the characteristic and algorithm of the method. Now to produce no other argument against this idle pretence, in Sir Isaac Newton's letter of October 24, 1676, M. Leibnitz knew, there was included this sentence "*Data æquatione quocunque quantitates fluentes involvente, fluxiones invenire, et vice versa,*" which was the substance of the two first problems of his treatise, written in 1671. How then is it possible to solve these two problems without a knowledge of what M. Leibnitz is pleased to style the characteristic and algorithm? Can it be imagined, Sir Isaac Newton had no marks for what he called fluxions? It is certain, that so long ago as 1665 he used those of p, q, r &c. to denote the fluxions of x, y, z &c. and afterwards $\dot{x}, \dot{y}, \dot{z}$ &c. for the same purpose, and also other symbols as occasion and conveniency required*. Must not therefore M. Leibnitz's assertion appear very unpolite, not to say worse, to Sir Isaac Newton; who though he had by him tracts written in 1665, 1666 and 1671, &c. replete with this characteristic and algorithm; yet never appealed to their authority, till provoked by M. Leibnitz's unfair proceeding in so freely using his own testimony in his own behalf?

HENCE I think it very plain, who was the real inventor of these methods; and as the accuracy of Sir Isaac Newton's demonstrations depends on the first Lemma of the Principia, this has been objected to. M. Huygens soon animadverted on it, which occasioned Sir Isaac Newton, in the second edition of his book, to make some alterations in the expression. Notwithstanding this, its sense has been mistaken

* Journ. Liter. 1714. p. 341, &c. also Philos. Trans. N^o 342. p. 204, &c. or Com. Epist. p. 37, &c.

by many. For which reason it was truly and fully explained by Mr. Robins; as also by Dr. Pemberton, he being called upon so to do. What Mr. Robins has written of it, is delivered in several places of the preceding tracts; and I shall conclude this Appendix with Dr. Pemberton's explanation, as it was published in the History of the Works of the Learned for January 1741.

LEMMA.

Quantities and the ratios of quantities, which constantly tend towards equality during any whatever finite space of time, and before the end of that time approach nearer together than by any whatever difference given, become ultimately equal.

HERE are supposed the following conditions.

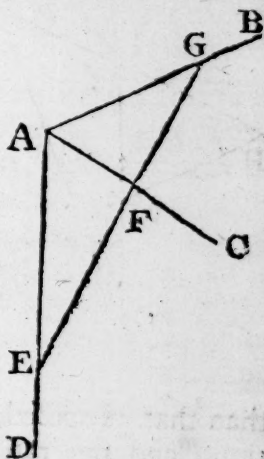
1. THAT the quantities or ratios approach more and more towards equality.
2. THAT they be known so to approach during a space of time, whether longer or shorter, which is sufficient to answer the subsequent conditions.
3. THAT whatever difference, how minute soever, shall be named, these quantities or ratios shall be known to approach, till they come within that difference.
4. THAT after they have passed that difference, they shall be known still to continue their approach.

UPON these conditions the quantities and ratios in this Lemma are said to become ultimately equal.

OR more briefly, varying quantities or ratios are here said to become ultimately equal, if they so approach, that, whatever difference be proposed, it can be shewn, that within a finite space of time, those

those quantities or ratios will come nearer than by that difference, and still be approaching.

For instance, if from the same point A three straight lines AB, AC, AD be drawn, and in the line AD the point E be taken, whence the line EFG shall be drawn at pleasure, it is manifest, that EG is greater than EF. But if the line EG be supposed to turn round the point E towards the line EA, it is also evident, that whatever difference between EG and EF be proposed, the line EG may move so long towards EA, till the difference between GE and EF shall be less than the difference proposed, and also the lines GE and EF shall still continue to approach.

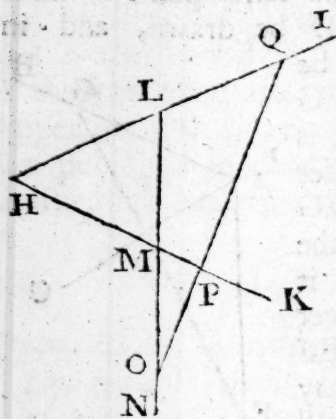


AGAIN, though the triangle GAE is greater than the triangle FAE, by the motion of the line GE, these triangles will also approach in the same manner.

FARTHER, if any polygon be inscribed in a circle, by the subdivision of the arches subtended by each side of the polygon, another may be inscribed of a greater number of sides, which latter will differ less from the circle than the first; and by the same means, a third polygon may be inscribed nearer in magnitude to the circle than the second, and the like process may be continued without end; insomuch that no difference how small soever can be proposed; but polygons may be inscribed, till that difference be surmounted, and yet other polygons still less different from the circle be describable.

MOREOVER,

MOREOVER, if the three lines HI, HK, LMN be given by position, and from the point O in LN



the line OPQ be drawn, the ratio of QO to PO will be greater than the ratio of LO to MO. But if the line OQ move round the point O towards OL, no ratio can be proposed greater than the ratio of LO to OM, how small soever be the difference, but the line OQ may approach towards OL, till the ratio of QO to OP becomes less

than that proposed, this line still continuing in motion, and the ratio of QO to OP yet farther decreasing.

In like manner the ratio of the triangle QLO to the triangle PMO is greater than the duplicate of the ratio of the line LO to MO; but by the motion of OQ the ratio of those triangles will approach to the duplicate of that ratio in the same manner as before.

In the last place, the ratio of LQ to MP is greater than that compounded of the ratio of HL to HM, and of the ratio of LO to OM; but no ratio can be proposed greater than this, which by the motion of the line OQ shall not be surmounted, before the ratio of LQ to MP ceases its approach towards that ratio.

Now in every one of these instances all the conditions expressed in the proposition are complied with; therefore by this proposition EG must be said to become ultimately equal to EF, and the triangle GAE to FAE, as likewise the polygon inscribed in the circle ultimately equal to the circle itself; also the ratio of QO to OP is concluded to be ultimately the same with

with the ratio of LO to OM, and the ultimate ratio of the triangle LOQ to the triangle MOP, the duplicate of the ratio of LO to MO; and lastly, the ratio of LQ to MP ultimately the same with the ratio compounded of the ratio of HL to HM, and of the ratio of LO to MO.

THE only difficulty, which here occurs, is, that whereas GE and EF become actually equal, both coinciding at last with the line AE, and the ratio of QO to OP becomes at last actually the same with that of LO to OM, the like is not found in any of the other instances; for the triangle AGE can never be in reality equal to the triangle FAE, nor the polygon by any subdivision of the arches be made equal to the circle, wherein it is inscribed. Again, the ratio of the triangle OLQ to the triangle OMP must ever be greater than the duplicate of the ratio of OL to OM, and the ratio of LQ to MP always greater than that compounded of the ratio of HL to HM, and the ratio of LO to MO.

BUT if it be said, that therefore in these cases nothing more can be concluded from the conditions proposed in the proposition, than that these magnitudes and ratios are not ultimately unequal; to this the answer is, that this negative conclusion is fully sufficient for all the purposes, to which Sir Isaac Newton has applied this Lemma, and that the only advantage, which accrues from the affirmative form of expression used by him, is some additional degree of brevity. And this affirmative form of speech is borrowed, not from the doctrine of indivisibles, but from the writers on geometrical progressions, with whom it has been usual to call the limit of the sum of the terms in an infinite progression, the sum of the whole series, though no number of terms in such a progression will amount to that limit. In particular, Gregory of St. Vincent, who avoids the use of indivisibles,

indivisibles", first defines this limit thus †. "Terminus [the limit] progressionis est seriei finis; ad quem nulla progressio [by the addition of its terms] pertinet, licet in infinitum continuetur; sed quovis intervallo dato propius ad eum accedere poterit;" and then in his exposition of this definition has these words; "Terminus igitur progressionis talis est, quemadmodum explicuimus, cum scilicet aggregatum, sive summa terminorum progressionis, quantumvis continuata, nunquam excedit quandam magnitudinem; excedit verò omne minus illa magnitudine, atque ita posset etiam dici productum sive quantitas totius, datae progressionis, et magnitudo illa æqualis diceretur toti progressioni datae; hoc est omnibus terminis proportionalibus simul sumptis."

The triangles GEA, FEA, and the triangles QOL, POM, also the lines LQ, MP, are by Sir Isaac Newton called quantitates evanescentes, or vanishing quantities, because they are supposed continually to diminish, till they vanish or come to nothing. If the motion be supposed to begin the other way from the line EA or OL, then Sir Isaac Newton calls those quantities quantitates nascentes; and the ratio, which is the limit according to the conditions specified in this Lemma, is called ultima ratio quantitatum evanescentium, and prima ratio nascentium, though the quantities never actually bear that ratio.

* See Prop. 45. Lib. de Duct. Plani in Planum. † De-
 finit. 3. Libri de Progression. Geometricis.



End of the SECOND VOLUME.

